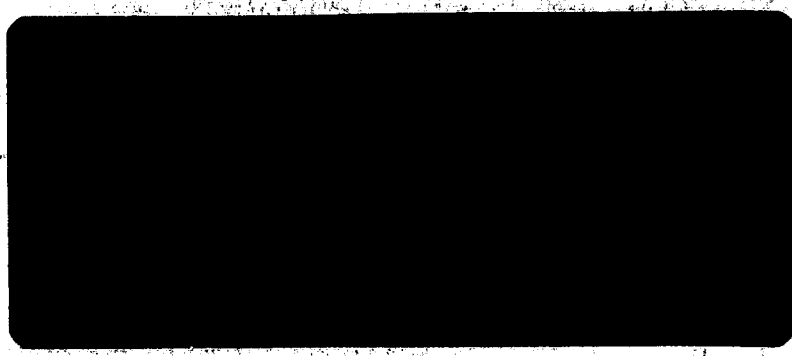


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RADIO SCIENCE CONFERENCE

W. H. Pickering et al

JET PROPULSION LABORATORY
California Institute of Technology
Pasadena, California

June 16, 1960

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PREFACE

On June 16 and 17, 1960, a conference on radioscience was held at the Jet Propulsion Laboratory. It was the purpose of this conference to provide an opportunity for those engaged in the NASA lunar and planetary exploration program to become better acquainted with the activities in the various branches of the field of radio astronomy.

The interest of, and the exchange of ideas by, the participants indicated that spacecraft studies could advance the vistas of electromagnetic studies, and also, that radioscience and spacecraft are naturally compatible.

During the course of the radioscience conference, many recommendations were made relative to radio experiments to be conducted in conjunction with the spacecraft. These recommendations have already influenced the experimental spacecraft program.

The conference was of considerable value to the Jet Propulsion Laboratory; it is hoped that all the participants benefited by the exchange of information. To this end, the unedited transcript of the first-day's session is being made available to the attendees for use as an outline of the work currently being done in the field of radioscience.

P R O C E E D I N G S

DR. W. H. PICKERING: Ladies and Gentlemen, I might as well introduce myself and get this started. My name is Pickering, and I just thought I would like to say a few words before starting in on this meeting.

I would like to first of all welcome you here to the Laboratory. I am very glad to have you visiting, and I hope that these couple of days will prove fruitful for all of us.

I would like to say that as far as I am concerned I feel that this meeting ought to be treated as an informal seminar-type meeting. We don't want to do anything any more formally than necessary. We would like to regard the meeting as an opportunity for people working in this area to know something about the problems which we feel we face in the lunar and planetary program, and to help try to arrive at some points of common interest and to explore some of the problems which we see and to perhaps find some new approaches which we want to solve.

I think, as probably most of you know, our primary function here at the Laboratory is within the NASA to carry out the lunar and planetary unmanned exploration, and this is a program, then, which is, of course, getting started which will result in vehicles flying to the moon and planets in the relatively near future and eventually,

of course, with landing instruments on both moon and planets.

It seems pretty clear that in the course of this program information which is being developed by people represented in this room is going to be of value to us. Techniques which are being developed here for Earth-borne instruments -- Earth-borne measurements, rather, can be applied to spacecraft to conduct measurements from spacecraft in the vicinity of and landing on the moon and the planets.

Now, Mr. Brown here, who has been handling the mechanics of this meeting, has suggested that we will start out with short discussions by Laboratory people on our technical interests and some of the engineering problems which we see associated with this, and then we go from there with opportunities for the various groups to discuss the kinds of things which they feel would be of interest, and then to discuss for us the kinds of interests which the various groups have.

I don't know that there is any point in making a sort of a round-robin introduction of people, but let me at least note that I see representatives here from -- let me just read a few of the universities. Michigan, ABMA, Cal. Tech., Stanford, Bureau of Standards, Ohio State, Space Flight Center, Bendix, Autometric, M.I.T., Cornell, Westinghouse, Harvard, Lincoln Lab., Space Technology Corporation, et cetera. So I think we are rather widely

represented of people in the country who work in this field.

I hope that one of the things which comes out of this meeting is a decision as to whether or not we want to continue an informal group meeting, whether we want to set up a formal symposium on the subject at some later date, or just what might be done.

I might point out, incidentally, that in approaching this meeting I think that here at the Laboratory we are approaching it with a full knowledge of we are quite innocent in the areas which are being discussed here. We are concerned with the problems. We believe we see some areas where we need the techniques and the information being developed, but we have not ourselves done any appreciable amount of work in this area. Our main work in the communications area, of course, has been with the problem of communicating with vehicles, and here the work which is done at Goldstone and at the Laboratory is, I think, quite outstanding; but this is a somewhat different facet of the problem than what we are discussing here.

I understand that it has been proposed that some of you would like to go out to Goldstone, and we can, perhaps, arrange this if you would like to see the facility throughout there on the desert.

Well, with that introduction, I think, Walt, that we will have first you and then Small talk a little bit

about the Laboratory's problems. Shall we start with you?

MR. W. E. BROWN, JR.: Well, actually, our interests are in two general categories. One is associated with knowing the properties of the surface of the moon and the planets and the atmospheres that are involved, and the other area is in general radio experiments that can be conducted in space on the conjunction with spacecraft that will help the theory, electromagnetic theory, gain ground just as analogous forms to the way cosmic ray was being done.

Actually, in one category some of the questions that we ask may be rather obvious, but we can run down a few here.

We are interested, for example, in the surface profile; what is it on the moon or on the planets; and what can be learned from the scattering pattern from limited areas, small areas, rather than just be looking at the whole planet once. Can the pattern be measured? Might you measure this in conjunction with a ground-based installation and a spacecraft working together? Can both radar and radiometer techniques be used to find this information?

Other things that we would be interested in: Whether or not you can find individual surface characteristics that you could use to help guide a landing in. Usually the way you pick an area, a spot to land on, you find that characteristic point and work from it in finding a place to

land. The place to land may not have a characteristic point.

Other things are the surface texture and the composition. How deep does it go? What's the bulk modulus? What's the temperature on the surface and beneath the surface? We have temperature balance problems if we are going to imbed a spacecraft in the material, and this business as to how deep it goes. For example, on the moon there are a couple schools of thought, and to quote Professor Gold from Cornell, he feels that this experiment we are planning to land on the moon is going to disappear in many layers of dust and we will never hear from it again. Whether or not this is so or not, we would like to be able to determine.

Then there are other questions like the electrical properties. How do they relate to physical properties? Are there electrostatic fields on the surface of the moon, for example, or on the planets would there be surface electron current or ion currents? Then in the atmospheric properties we would like to know is there any precipitation. When a radar looks at the disk of Venus does it see a variety of storms going on; precipitation taking place which may not reach the surface of Venus but still would mix up the echo. And what are the wind currents, and that sort of thing?

Another area is the velocity of propagation not only near the planets but between the planets. That is the velocity of propagation for electromagnetic waves.

In the second category, as for experiments, what experiments are appropriate to conduct in conjunction with spacecraft? Such questions are -- for example, would it be a good idea to look at the Earth by radar from very high altitudes? We might find that over a large land mass of the Earth we get up at a high altitude and look at the Earth and we will find that it is smoother than the moon. Would an experiment on the Venus spacecraft that is going to fly by Venus be worthwhile, a radar to measure the spin axis and the spin rate, or can we surely measure these things in the next year or so from the ground?

Now, when it comes to these experiments, those that are large in size have to be pretty well known before you design spacecraft, because spacecraft, more or less, has to be designed around it.

This leads to some schedule problems, of course. For example, if we want to fly an experiment in 1962, we have to have hardware by next summer, a year from now. It means that we have to start advanced development this summer some time. Well, what sort of thing should we do? This is the area where we need good advice in, and you see there is a mode of operation involved here. We would like to have

the people who are most competent in the fields or adjacent fields be able to advise us what to do.

This is why we are interested in what you think could be done. We are also interested in your general area of work because we may recognize areas that we are interested, similar to this sort of thing.

Now, if we develop an experiment it may not even fly. It might not get on board a flight, which -- well, it might not get on the flight you plan on getting it on; but I think Dr. Tice, when I talked to him on the phone at Ohio State, pointed out a very good thing. If you have a very good experiment and it is not marginal, chances are it will fly are very good.

DR. PICKERING: Thanks, Walt. I would like Johnny Small from the Laboratory to say a few words about the sorts of engineering problems that are associated with the space work.

Johnny?

MR. JOHN SMALL: Basically I am going to talk about the problems that the engineer has, and my background in particular work now is what we define as system engineering where it is that putting together the different capabilities in order to make an over-all experiment, but primarily based upon the spacecraft itself or the vehicle

which will be the means of getting to the point. It is a means of transportation, and it is a means of sending back the information, and an engineer basically is interested in the information that he needs in order to accomplish a job as one criteria, but he is also more interested in accomplishing a job than actually learning about the details of the job; so that he must take the available information that he has to do the design and will very often attempt to design around conditions which are unknown or to put in a solution which doesn't require as much information.

An example of this type of function is where in a spacecraft you operate in a vacuum which we don't have on this Earth, or can not simulate easily; particularly in the Explorer two or three years ago. So you might seal the rocket motors that you are going to use in outer space because the ignition problem in the vacuum might be the kind of problem that you wouldn't know exactly how to handle, but to be on the safe side you might seal the motors.

Another type of thing that an engineer must do is compromise between the schedule, between the money, between the requirements of different people in order to accomplish a step in an over-all program or to accomplish one particular mission, and he does like to have, on the other hand, a goal or a carrot in front of the donkey, if you like, out ahead of him in the direction he would like to go in the future,

so he would like to know the future things that people would like to do as a result of the experiment or in their own minds, even know that they may have to compromise in a particular experiment, to get some information.

An example of this that always faces us is how much band width everybody gets for their experiment and how much band width the engineering part of the measurement has to have. We all want reliable spacecraft. We all want them to work the first time, or we make the statement, "We ought to have developed vehicles." This requires a lot of information relative to all the functioning in vehicle, and our only means of getting this is in the actual flight condition. The actual environment is through telemetering; so an engineer wants some of this as well as the scientist wants it for the actual measurements he is going to make, so there is a trade off to be made between this type of information during the mission.

Of the types of design conditions that the engineer would like to know relative to future operations, limited to the planets and the moon, I have listed four in the order I think they make the most sense, and I think the planetary environment has to come in as a first item in this, in that we do not -- let's call it a re-entry model into a planetary atmosphere, we would like to know the best guess of what this environment is during the descent, and in other words

we want the experimenter, in a manner of speaking, to say what he expects to get or what he expects the environment to look like in order that we can design the equipment; yet we might put in a factor of two or three in some areas or do the thing in a manner that would get around the particular environmental condition, but we would like to know that first approximation.

And the second would be the lunar surface itself. And inter-lunar surface conditions we are more interested in a statistical distribution for possibly small areas than we are in a whole picture of the entire moon or that there are certain cliffs or this type of thing. We are more interested in local conditions with statistical numbers attached to them because if there is a 90 percent probability that we will only have a slope of five degrees we would design a landing system in one particular manner. If, on the other hand, there were discontinuities with some other manner on some other confidential level we might do it in a different manner entirely. So, as in most environmental conditions, you -- except possibly things like the temperature from the sun -- you do this on a statistical distribution rather than on a finite number that you use.

And then the planetary surfaces I put third because of the -- in time it is quite a distance away, and

the re-entry problem probably comes a lot before the surface problem.

And the last, the interplanetary environment, mainly from the little experience that we have so far.

To go backwards in the problem a little bit on Explorer, for example, there was this coordination that you did have micrometeorite gauges on the Explorer; yet in the design we had numbers that disagreed by -- oh, I don't know -- five orders of magnitude on the number of meteorites you might expect, depending upon who you talked to -- and the problem at the worst limit didn't look like it was capable of solution, and at the least limit it didn't look like we needed any solution.

So, being engineers, we took the one that was the easiest way out because of the pay load requirement. We wouldn't have had any pay load if we had taken the most conservative situation relative to this particular problem.

So this is basically the engineer's problem, is to almost know the answer for -- let's say 95 percent of the measurement before he starts the design; and so the work that you do and the work that you do from the Earth or Earth satellites or other types of conditions, if this can come out as statistical distribution of certain physical phenomena it helps the engineer in his design problem.

DR. PICKERING: Thank you. I said I wanted to

keep this as an informal meeting. Having arrived at this point, before I invite the various members of the audience to come up and say -- talk about their interests, I wonder if there is any reason for having discussion. Would anybody like to raise any questions about the Lab program or anything that has been said so far or what we are going to do today?

Why don't we go ahead, and I expect we will get to discussion later on.

We have arbitrarily selected an order of presentation here, and the first presentation, then, -- the first discussion would be by Professor Reintjes of M.I.T. to speak about space radar systems.

Professor Reintjes?

PROFESSOR J. FRANCIS REINTJES: Perhaps I should introduce myself, say from where I come and how we fit into this conflux of people who are here.

I represent the Electronic Systems Laboratory at M.I.T., and we are a laboratory group operating in the Electrical Engineering Department at the Institute.

By Institute I mean M.I.T. I always remember during World War II getting into discussion at the radar school back there, and this gentleman was talking about Tech., and he was saying a lot of things that didn't make sense to us, and finally somebody said, "Are you talking about Cal. Tech.?" He says, "Is there another one?" (Laughter.)

Also there are some folks here -- if I may just take a second to say so -- from Lincoln Laboratory which is part of M.I.T. also, which is a separate laboratory not operating under any specific department, but under an Institute jurisdiction, and I think you probably have heard quite a bit about it.

I would also like to express gratification to the Jet Propulsion Laboratory people here for getting together and taking the initiative in getting together people with this kind of interest in space technology and scientific experiments. I am among those who can talk about things we are interested in doing rather than things we have done, and I am sure there are people around the country in the radar astronomy group and radio astronomy groups and optical astronomy who have taken looks and listened to things happening from the far distance places and have more experience in this sort of thing than we do.

Getting together all of these people, I think, is significant and contributes significantly to a program of this kind. Our Laboratory, over the years, has been interested in three basic areas of data gathering, data processing, and control; and the aspect of our Lab which would be most significant here, I think, is the work that we have done for quite a number of years in air-borne radar systems.

I would say that the spacecraft radar is a logical extension of this kind of work. It is a real extension of it, and this will be the graduate course, I am sure, to get the systems which will be useful and really can get the kind of information that we need.

It seems to me that in talking about spacecraft radar problems there are three aspects to this problem. One is the identification of significant and useful key experiments that can be performed within a time scale that can do people some good. I don't think just engineers at spacecraft radars -- I think you have to have at least some goals in mind -- some experiments, some specific experiments, to perform.

Having made an intelligent estimate of the chance of successfully performing these experiments and identify them; then there is this matter of organizing the system which will yield data of -- confident data on the results, and that is the second aspect; and I think the third aspect is the development of components which will insure satisfactory operation of your system and success of the experiment, and I feel that there are some unique componentry -- types of components that will be required here because of the specialized nature of these experiments. I am thinking here of the environment which the gear will have to survive in, and it is a kind of a chicken-in-the-egg proposition here

because to intelligently design the system you would like to know the environment you are designing the system to gain information by.

And then there is this other matter of -- I think we are getting into the area of having to design systems for what I call a long mobile shelf life which in the equipment is not really needed and doesn't have to operate for quite a long time after you have sent it away, and the question is now how can you be certain the very first time that you turn this thing on it is going to give you the data that you want.

In thinking about experiments that might be performed with spacecraft radar we have thought that perhaps using radar to look at the cloud cover of Venus and try to identify what its texture and composition is and also what is beneath it might be a good role for spacecraft radar to serve.

I think -- with respect to Venus, I think there is a uniqueness for radar application here. Perhaps on lunar experiments you might get by with other kinds of centers. Either -- well, optical centers, either the visual or non-visual portion of the spectrum.

With respect to Venus, I think you have got a different situation here in the lower frequency electromagnetic portion of the spectrum. I am thinking below IR

and things of this sort, but still high compared to what we would call radio communication. It might serve in good stead.

Actually, we don't know what frequency to pick. At least I don't. I hope maybe I can find out before the day is out. Maybe the land-based people with radars have gotten some data that can help us out on this, and if so I think that this kind of meeting is tremendous, if we can swap information of this kind.

Certainly you would like to go later into map-making kinds of experiments which can identify fine grain structure of any surface beneath the cloud cover of Venus. How successful you will be in this I think depends upon how much equipment that you can put aloft, and you see there is always in this background the idea of -- well, two things, as I see it. The watts per pound that you can radiate, and then the other problem is just how many pounds can you put up into space.

Well, with respect to what data you can get from a near miss in Venus, I am not so sure. The 20,000 -- I just saw this second letter which apparently I missed because I left early. A mere miss of 20,000 kilometers seems like a long distance, doesn't it? So for radar to work I think maybe even a soft or hard landing directed directly toward the planet might serve initially of more value.

Later on hopefully an orbiting-type of vehicle might pay off,

Well, I could continue on and identify the problems which one will have in assembling a system of this kind. I am sure that you are all familiar with them if you have ever tried to build systems that have to operate reliably. The choice of system parameters; the antenna problem is an interesting one for a spacecraft radar; and then I think, too, that in any design of a system of this kind one should take a real hard look at not only data gathering but data retransmission as an integral package, and it isn't clear in my mind whether these are separate systems or whether they are the same system. It depends on how your experiment is organized. If it is a prime remission plus a secondary remission you might want to keep the retransmission gear separate so that you can be at least assured if the primary equipment conks out, why you will get something back. On the other hand, if you can be assured that you can engineer reliably and it will perform as you want it to, then I think there might be an opportunity for integration between the transmission and -- the data gathering, let's say, and the data retransmission equipment. This comes about only through intelligent appraisal of the system engineering problem.

Well, with respect to -- then -- the reliability problem, I would hope that some of the work that we have

been doing in our laboratory toward the development of solid-state devices which seem to have a degree of reliability, certain aspects of radar -- I'm thinking particularly of the modulation field -- might be certainly applicable here. The problem of using hard-tube switching or soft-tube switching, I think, and the problems associated with them, might be circumvented through solid-state magnetic switching devices. There is a need here for real reliable and light-weight switching because some of the magnetic materials we have, although yield reliable switching at the moment, I think they are competitive weight-wise with other kinds of high-powered modulators, but we are not beating the weight problem as much as we should, and I think in any program of this kind probably to aim in component development for an order of magnitude -- and by that I mean ten, factor of ten -- improvement in the radiative watts per power at least should be a goal, and packaging densities hopefully should go up appreciably beyond that which we are able to have.

I can ramble here -- I hope some day I can tell you results rather than blue-sky thinking, but I hope in the few moments that I spoke in here -- I have tried to identify first, some of the problems which I think we are facing, and secondly, what some of our interests are in our own laboratory.

DR. PICKERING: I think it might be of some value to put down what we are talking about schedule-wise, the sorts of things which we are talking about.

'61-'62 will probably be thought of in terms of the moon, and we have this proposed capsule program now which will land with a hard landing. At the end of '62 is the first planetary opportunity after the current one this year. So here we have both Mars and Venus opportunities, and it is pretty clear that spacecraft which are sent to the planets at this time will be relatively simple; however, I would like to point out that in this entire program we are talking about spacecraft which are attitude-controlled so that their attitude stabilized can point back to the Earth and point of planet and so forth.

And then the next development in the program will be in the direction of soft landing on the moon. The next significant planetary opportunity comes in '64 which again will give you the planets, and now presumably one has a chance to do something somewhat more elaborate with the planets; where one can talk about a precision close in orbit around the planets at this time, or a separable capsule, or what it might be. I don't know, but at least you see that this is a period when the first time that you will be able, perhaps, to get close to the planets, or stay close to the planets.

PROFESSOR REINTJES: This is fixed by the availability of the vehicle?

DR. PICKERING: No. This is astronomy, and so is this. These dates are astronomical dates. I suggest these as being relatively simple experiments on the basis that there really isn't a great deal of experience that we are going to have before this time.

PROFESSOR REINTJES: Right.

DR. PICKERING: And also the vehicle capability will not be too large at this time.

When we get down here, though, we begin to talk about larger vehicle capability, considerably more experience, and therefore presumably such things as orbiting or possibly landing. Landing a capsule; not a soft landing.

Well, this program, then, will evolve. I think Professor Reintjes brought up an interesting question here, and if one is going to talk about this sort of a system, perhaps one of the most difficult things would be to decide what kind of data retransmission would be needed and how it should be done. What sort of band widths for the data retransmission system? What kind of data should be sent back? What fraction -- as Mr. Small pointed out here, the communication system will certainly be required to send a variety of information back, and how much of it should be allotted to a radar?

PROFESSOR REINTJES: Yes, there is a process of how much you want to process upstairs before you send it back.

DR. PICKERING: Yes, because of the problem of sending data over a hundred million miles one would like to minimize the amount you send.

MR. BROWN: Dr. Pickering, do we know what band widths are available for some of those experiments?

DR. PICKERING: Karl, I see you here. Do you want to quote a number? I don't know. Well, let's say -- of course one doesn't know yet exactly what powers and antennas are actually going to be available on the spacecraft particularly. I think we know pretty well on the ground what we are going to have, but the spacecraft antenna design clearly is tied up with the problem of building a large antenna on a spacecraft after you get it out into space, and the power requirement is tied up with the question of how long do you want the transmitting to be operating. Is it to be a continuous or a switched-on sort of a system? I think we can talk, though, in this region. Band widths on the order of -- what? Hundredths of cycles. Am I right, Karl? And this, the order of thousandths.

A VOICE: About a kc, I think.

DR. PICKERING: I mean this is the sort of thing.

A VOICE: I think the general thing is the total

number of bits generated, and then -- when looking and planning an experiment you look at the -- have bits generated rather than the band widths.

[Discussion off the record.]

DR. PICKERING: This, of course, does pose another interesting problem, as pointed out, that at this time we can not guarantee a mis-distance of less than some tens of thousands of kilometers, and the question is what can one do with the weights which are measured, or possibly hundreds of pounds, but not very many. Mis-distance is the orders of tens of thousands of kilometers. Here the weights should go up significantly in the hundreds of pounds, and the mis-distance presumably much smaller.

Looking at the planets as being -- as pointed out, perhaps the most interesting problem here is what can you do with the radars on Venus. Of course as someone else has pointed out, I think, -- it has certainly been pointed out at other times -- when one embarks on this kind of a program the costs of the program, of the flight program, are very large. Obviously one wants to answer whatever questions one can from the effort at more modest costs before you start flying out in space.

The next man we shall hear is Dr. Moore from the University of New Mexico.

A VOICE: Mr. Edison is going to present the talk.

DR. PICKERING: Thank you.

Mr. Edison?

MR. ALLEN R. EDISON: Dr. Pickering, Ladies and Gentlemen, the radar return program at the University of New Mexico has involved the investigation of the reradiation characteristics of terrain at near vertical incidence and using frequencies of 450 and 3800 megacycles and a narrow pulse radar. About 30 different target areas over the United States have been investigated. These data were taken by Sandia Corporation starting about 1951. Since this date we have been analyzing the data, and for the most part this stage of the work has been completed at this time.

The experimental results that were obtained from these investigations involves the dependence of back scatter on the angle of incidence; the determination of Finnell reflection coefficients for various types of terrain; the range of fading that is involved in the return signal; the rate of fading; a determination of the total path attenuation, and this is defined in a special way for the pulse-type situation; and also a study of the variation of signal strength with altitude.

All of these factors, I think, are important in an interplanetary radar study, plus some additional factors such as propagation of the electromagnetic energy through an ionized atmosphere on one or both of the planets.

The problems faced in the interplanetary radar program are not entirely new to those who have been studying the radar terrain program as involved with the Earth. At least the theories in the daily reduction techniques that have been applied to the terrain return studies will also be applicable to the interplanetary studies we properly extrapolated.

I would like first to define a couple of terms, and as these terms mean slightly different things to different people, and we need to know exactly the definition that is being applied.

The first term has to do with the specular power or the specular signal, and as a definition I would say that the specular signal is a return based on image theory, which is quite well known. We could also say that the specular is a coherent type of signal, and this implies that it is also a non-fading type of signal. This non-fading portion, I think, is quite important. The appearance of a specular type of signal on an A-scope and with the A-scope presentation is characterized by the return having the same appearance as the transmitted pulse; that is the same shape. If your transmitted pulse is a tenth of a microsecond your return pulse would also have the same width in time.

The surface requirement is generally considered to be a large flat surface which involves -- or which is

flat over several Fresnel zones. The exact number is, of course, not determined exactly. However, it has been shown that a specular component can be resolved or obtained from a rough or a scattering type of surface, so we may have a specular component -- and we do have a specular component in our surface, is completely rough.

Then a final statement is that the variation of power with altitude would be inverse square with distance. Now, in the case of the specular power return -- and this, of course, comes from the image theory.

Now, the next term is the scattered power or scattered signal return, and the definition here would be that the scattered signal or scattered return is an incoherent signal, and since it is incoherent -- that is, the phase is not related in any particular way to the phase of the transmitted signal, -- this incoherent signal will fade. If we look at the scattered signal on an A-scope we will observe very rapid -- in most cases very rapid fading from one pulse to the next.

Another characteristic that is quite well known with scattered return is that the pulse will be stretched in time. That is a short transmitted pulse will return to the receiver as a considerably longer pulse.

Now, at this point I would like to show four slides that I have taken from data that were obtained from

the Sandia Corporation experiments to illustrate some of these effects.

If I may have the first slide, please.

In this slide I have shown at the top -- a transmitted pulse. It is the pulse that is slightly larger in amplitude. The amplitudes that are shown here do not have any particular significance insofar as the transmitted pulse is concerned. The transmitted pulse that we see here has been corrected to account for the receiver band width, so this was called a receiver-corrected transmitted pulse shape. This is the envelope of the tower.

Then superimposed on that is a median-received pulse. This median pulse is obtained from analyzing the data over -- involving several hundred, or usually several hundred return pulses.

Then in the column below I have shown six or five return pulses. These are consecutive pulses as traded from a 35 millimeter film record, and we see that the time interval involved between the pulses is 2.57 milliseconds, and the radar was moving in a horizontal flight path at a velocity of 135 miles per hour.

So these return pulses were taken in a space a distance of $15 \frac{1}{2}$ centimeters or .155 meters apart in space.

We notice that there is a 6 db range of fading from our data on the right. This 6 db range of fading that

we show here applies to the peak of the pulse. We have selected that particular point to calculate this range of fading. The fading here is not very severe. As we see, the pulse indicates that there is some scatter return and out on this trailing edge we see considerable variation in the shape of the pulse. We would attribute this to the scattering.

Now, the target that is involved here is in a sandy area in Nevada near Yucca Lake. The power return is primarily specular with a fairly small amount of scattering. The fact that the range of fading is quite low would be an indication of a primarily specular type of return. This is one of the few targets that has shown such a narrow range of fading. In the majority of targets, including desert, it has shown a larger range of fading.

May I have the next slide, please?

This is a flight over the desert near Salton Sea in California, and you are familiar with the type of terrain involved here. Here again we have the transmitted pulse and the median-received pulse, and in this case we notice the range of fading is about 20 db. Now, this is quite a large range of fading. Again that applies to the peak on our pulse.

We notice that the peak value does vary widely from pulse to pulse, and since these are consecutive pulses

taken from the film they occurred close together in time and are samples, something like three centimeters apart in space. So the fading is quite rapid.

May I have the next slide, please?

This shows the same target area with the received pulses as superimposed on top of each other to give you a little better picture of how the fading occurs from one pulse to the next. This is the same data that we saw on the previous slide.

Now, may I have the final slide, please?

In this case I have sketched a scattering cross section curve. This shows the radar cross section per unit area of the terrain as a function of the angle of incidence where zero is taken as straight down. For a very rough surface this curve will be quite flat.

In that case if we have a transmitted pulse, represented by this curve, the median-received pulse will be stretched in time considerably because of the scattering. The increase in length of the specified pulse will be, of course, somewhat determined by the geometry of the situation and the antenna pattern that you are using, but it does have the characteristic switching, and there would be no argument about this being a return from a scattering surface. If we looked at individual adjacent returns we would find that they would fade widely about this median value.

Now, in the second case we have a scattering surface that has -- that is slightly rough, and the scattering cross section varies quite rapidly with the range of the incidence.

Now, in this situation, since the scattering cross section diminishes quite rapidly with the angle of incidence measured out from the vertical, the return pulse will not be stretched nearly so much. We receive smaller amounts of power from this region, and the return pulse -- the median has an appearance very similar to that of the transmitted pulse.

Now, the chemistry would be to say that this pulse looks like the specular because it is about the same in appearance as the transmitted pulse. Actually if we observe a sequence of these pulses we will find that they fade rapidly. They are not a -- it is not a true specular pulse as we have defined the term, and should not be considered as such.

So merely the fact that a received transmitted pulse looks like the -- or has the same shape as the transmitted pulse does not imply that it is a specular return. It is a scattered return coming from a surface that is not quite so rough as what we have considered in this case.

Now, the fading range that has been observed from most targets over the Earth is extended from 12 to 20 db. These targets, of course, were selected as relatively homogeneous. Large hills and mountains were not included. For the most part the target areas were smooth. They had different types of ground cover, however. In some cases they were cities and some cases lakes and oceans.

The fading rate that we would expect from a perfectly rough surface, one where the phase angle is uniformly distributed around entire 360 degrees, we would call it the Rayleigh-type ground. That is, the fading statistics of the envelope would follow the Rayleigh distribution.

Now, the scattered return has a variation in power with altitude that is inverse square with the -- or for the beam limited situation. It is approximately inverse cube for the pulse with limited situation. That is where you are using a narrow pulse device, and over many runs the experimental value for the power variation with altitude for scattering from the Sandia data is one over H to the 2.6 power, $4 H$ is the altitude. This, of course, is based upon the relatively low altitudes at which this data were taken. That is between two and twelve thousand feet, and, of course, if we consider scatter power from a single scattering facet the variation with distance is inverse 4th

power which you obtain readily from the basic radar equation.

Now, the separation of specular and scattered signal is fairly important, and as I have mentioned before it can be shown that the return from a rough surface can be divided into a specular and a scattered component, and this involves a separation factor which is an exponential with a negative exponent, and the standard deviation of the vertical heights appears in the exponent. Using the characteristics of a typical desert target extrapolated to the moon we get a gain of about 20 db or 20 db advantage for specular power over scattered power, based upon the different variations with range.

Now, high altitudes over the Earth -- I think the extension of the low altitude theory -- or the extension of the low altitude theory to high altitudes where curvature must be considered results in a relatively minor modification, and I believe that these extrapolations will apply directly to the planets.

An experiment involving return from high altitudes over the Earth it seems to be a very effective and perhaps the easiest way of obtaining information on the planets. Data taken at perhaps one hundred thousand feet would check the extrapolations of our whole altitude data, or an experiment involving altitudes of -- say, two to four hundred

miles would also be quite valuable.

Along the same lines, we have developed an ultrasonic simulator that we think has considerable promise in evaluating questions of this type. The simulator is to model electromagnetic reradiation using sonic waves in water. In this simulator both time and distance will be scaled to make the analogue computer quite versatile.

Phenomena that depend only on relative phase shift can be studied quite readily with a device of this type. Of course phenomena associated with polarization effects can not be investigated because of the stellar nature of the sonic waves.

This instrument or this equipment is presently available at the University of New Mexico, and we hope starting next week we will obtain some experimental results from it.

In conclusion I would like to say that the techniques used in ground studies plus some additional techniques, of course, and refinements, should be applied to the studies of radar return from the moon and the planets. That is, we want to investigate the scattering versus angle characteristics; the separation of specular and scatter power; the fading spectra of the power return; the various distribution functions, probability functions, that are obtained; and from these estimates -- and these will have

to be just estimates, with certain probability confidence limits placed on them -- estimates can be made of the dielectric properties and the roughness of the surface.

The relative distance variations of specular and scatter power must also be considered when analyzing data from the moon or planets. Present indications are that the fine scale moon structure -- that is the same scale that is observed at low altitudes over the Earth -- is somewhat obscured by quasi-specular scatter return from large rough facets on the surface.

I think we must extrapolate the existing information with the aid of some high altitude experiments over the Earth. We realize that these high altitude experiments will be much cheaper. We would be able to perform them at an earlier date, and I think they will be quite valuable as a stepping stone to the more difficult study. Also we think the acoustic simulator holds considerable promise as a means of resolving theoretical questions, checking extrapolations of present theory, saving time and money by doing experiments under control conditions in the laboratory, and also aiding in the design of perhaps the first lunar altimeter that will be used for making soft landings.

The easiest way to obtain maximum information from the moon and planetary echo is to extrapolate from

known properties of Earth echoes.

Thank you.

DR. PICKERING: If you talk about doing high altitude experiments over the Earth these would presumably be done from rockets. You would be traveling at quite high speeds essentially all of the time you would be taking observations. Of course this has also been true on things going by a planet.

Are you going to be able to analyze the data which you get from very rapidly moving objects?

MR. EDISON: Of course if your radar is moving in the vertical direction you have a problem of non-stationary time series involved which is a little bit difficult to analyze. What you need to do is break it down into a series of stationary time series. That is, consider the data from a relatively short part of travel, and I think the useful information could come from this even though the velocity is rather high.

[Discussion off the record.]

DR. PICKERING: Next on the list we have Ohio State. Dr. Tice is head of the Antenna Laboratory at Ohio State.

DR. THOMAS E. TICE: Well, I gather that the thing which is primarily in order is an indication from me at this particular time -- I'm going to try to narrow this down to

something intelligible.

I think we would like to say that we are interested in the surface characteristics of first of all the Earth, where we now are, the moon, where we may some day be; and planetary surfaces, who knows?

Now, the question is what surface characteristics are we talking about Obviously, many. But two categories -- one, electromagnetic; and two, physical. Now, specifically the electromagnetic properties that we are focusing attention upon are the scattering coefficients at all angles. This is first, and by this I mean the total scatterings of the energy for a given wave impinging at any given arbitrary angle of the total scattering information; and second, the emissivity for radiometric coefficients, and at this point I would like to interject a thought that the emissivity is completely determined by the total scattering characteristics, and this is one reason for being quite interested in those by-static or total-scattered characteristics.

The second category of general properties mentioned were the physical characteristics, and we believe the main one which has received attention there is surface roughness, but along with this general surface structure and composition, and if you like throw into this category dielectric constant and conductivity with which we perhaps think of, perhaps, as bulk electrical characteristics.

Now, to try to summarize, then, what we are interested in: we would like to be able to take a given surface, whatever it may be, and predict its electromagnetic scattering and emission. In particular, of course, we are interested in rough, semi-rough, or semi-smooth surfaces, since these are the ones we'd normally encounter physically; and second, we would like to use measured scattered data and radiometric data to determine those same surface characteristics when they are not known. In other words, the lunar or Venus situation, as the case may be. We might express, then, as the use of electromagnetics as a diagnostic tool in this case.

So, to kind of summarize, we are interested in electromagnetic and physical characteristics, their interrelation in trying to deal with them freely from one to the other with a good firm theory backed up by experiments.

Now, I would like to illustrate our interest in these, perhaps, by noting some of the things which have taken place in the past which we believe extrapolate into the future as well.

How would we learn something about surface? Well, one way, of course, would be by the use of active radar. So, if I may have the first slide, please, --

Active radar -- again, we are interested in both theory and experiment in determining a scattering back to

the radar set, back to the transmitter in the first place, and this is what we look at, a back scattering situation.

Our approach to this has been the following:

You see a truck with a hydraulically operated boom. These are two radar units mounted on here. They are independent. You only have to have one at a time, but we have units which can be put in at X case (inaudible) and four millimeters. They may be tilted in arbitrary angle incidence and then pointed down at some arbitrary surface. These are designed to simulate or approximate very closely a plain electromagnetic wave illumination of the surface portion which is illuminated with appreciable power.

In other words, you may say we are obviously not at a hundred thousand feet, or whatever it is, but nevertheless these are not designed as far field radiators, but merely designed to focus some 30 feet or so below the surface, which is a good approximation of plain wave illumination over the portion which is actually illuminated.

In the same picture you will see two types of surfaces which we have considered. The rough surface simulated by grass, on the one hand, -- and this, of course, we have taken on up to the corn and wheat fields and such like, -- and an asphalt surface, on the other, which we will consider as examples of semi-rough or rough for the grass and semi-smooth or smooth for the asphalt.

Now, by way of a comment on this particular operation, the usual procedure is to drive the truck along at a relatively slow speed. This gives the Doppler shift, which we make use of in this case, Dr. Pickering, to avoid some other complications, and then we can separate the reflected signals from the surface very easily back in the microwave equipment used in this Doppler-shifted signal.

So this is the equipment which we can use to measure the back scattering from the various types of terrain.

The next slide, No. 2, shows some results, both theoretical and experimental, obtained for asphalt, using, of course, the truck for these particular measurements, and comparing with the theory developed by Dr. Poake. This is a scattering coefficient, if you like, on the left. It has a function of angle of incidence. You might note that our data in many respects is complementary to the University of New Mexico in that we have focused attentions primarily away from vertical down near grazing, and they are primarily near vertical incidence.

The correlation here, first of all, is for the X band and the case of A band, measured and calculated data indicated there. The measurements -- or rather calculated value is indicated by the X's and zeros and the measured value is indicated by the curve.

Now, this is asphalt which is a relatively smooth surface. Try to keep the general shape in mind as we turn to the next slide which gives a relatively rough surface. You will notice it is much flatter in the characteristic return as a function of the incidence angle. Fairly flat, and incidentally before and after rain, which happened to be one of the parameters which we were studying at that particular time.

This, in other words, is two-inch grass, frequency case of A band around 35 kmc, if you like. So you can see a much flatter return as was indicated for the rough surface as compared to the fairly smooth surface.

The next slide I would like to show illustrates a point which we think is exceedingly important in relation to the space program. This is our calibration method for this particular type of equipment, and we think that calibration, getting a good accurate calibration, or the missile shots or the spacecraft warrant measurements will be one of the most severe problems to confront.

As we indicated, we need an absolute standard, or we wanted one, at least. We used a Doppler-shifted signal coming back normally. The way we achieve this in this particular case is to have a corner reflector out here which was rotating around on an arm. This gives a Doppler shift the proper frequency. Comes back into ray ourselves,

and gives us -- we, of course, have the known radar echo of the corner very accurately. We get the Doppler shift by rotation. This gives us an absolute standard for this measurement.

If we want to emphasize, though, the general principle that we think this question of absolute calibration is of exceeding importance when we go to the entire altitude spacecraft tests, and perhaps one should think in terms of trying to do it with some authority when you get up there. I think that is something that should be given a lot of thought.

Well, this now we have tried to illustrate so far has been the active radar or back scatter, if you like.

The next, we said we wanted to focus attention on the total scattering. The next slide, please, -- and again both theory and measurements involved, and now frankly by-static or scattering at various odd angles, as indicated here by the sketch, is of importance in and of itself; but even, perhaps, more so it is important because it is the key to the emissivity and radiometric coefficients.

In other words, this particular is very important. This now would be db below direction transmission, between the two antennas, transmitting receiving. We are keeping status of R fixed -- one of the angles fixed, if you like, -- in varying because of the status of T in what are described

are a -- two polarizations, a horizontal and vertical, again, for two-inch grass. This happens to be at 1150 megacycles for two particular instance angles in comparison with 16 and 11 1/2 degrees.

I notice the rather marked variation in degrees scattering coefficient as the angle faces the area. This is the two-inch grass relatively rough surface.

Now again, though, this is an illustration of some by-static measurements of rough surfaces which lead us to the next major area which we think is important.

The next slide, please?

Emissivity in radiometer. Now, this is temperature plotted against the instance angle at 35 kmc. This happens to be horizontal polarization. The circles and crosses are the calculated values from Dr. Peake's theory, and the solid lines are the measured values.

Again you see here, in comparison here, of the radiometric conditions coefficients for the asphalt road, the solid, and the dash line representing the two-inch grass, and you can see some indication, at least, of the correlation between theory and experiment. At least the trends are certainly there.

So again, looking back, active radar, or back scattering, by-static scattering, or scattering at various angles and emission illustrate the types of things which we

would think of doing in predicting the scattering and emission from typical rock, semi-rough or semi-smooth surfaces.

Now, let's take a look, perhaps, at the use of electromagnetic data to determine the surface characteristics. Up to now we said, "I have a given surface. What are scattering and emissivity?" Let's look the other way now. "I know the scattering and emissivity from measurements. What can I do as far as predicting?"

At this point we may be open to some correction. What we have attempted to present on this slide is some data obtained from some other people, and they certainly know their data better than we know their data.

The dashed line at the top, of course, is the radar or rough surface from the moon measurements made by Stanford, as we understand the data in the published article. The solid line is the data measured by M.I.T., as we understand their published data; and what we -- Now, that's the solid line along here which has been interpreted as Lambert's Law by Pettengill and some others, megacycle data. So first is the dash line from the Stanford data; the solid line from the M.I.T. data; as we understand their data, and if I get through here and they want to correct me, please feel free to do so.

What we have superimposed on this is some

calculations made by OSU, and these indicate vertical polarization.

H's represent horizontal correlation, and as we understand the experiments which were performed, they actually -- by Stanford and M.I.T. -- they involved a combination of both.

Now, what we simply tried to point out here, then, in other words, for one thing it would appear that the spread between vertical and horizontal may well be one explanation of apparent discrepancies in experimental data; and second, we would very much like to see an experiment performed with controlled polarization as it relates to the incidence plane at the surface of the moon.

In other words, what we have calculated has been the coefficients we would expect if it were all vertical or all horizontal, and presumably other values would come in between somewhere.

What we would like to see one of these groups, perhaps, perform, or a similar group, is measurements where it is an accurately controlled vertical polarization at the position on the moon where the energy impinges, at the limited portion of the moon.

Well, perhaps we can go into this in more detail later if this is appropriate, but this, for one thing, illustrates, I think, an attempt to correlate -- measure

data, on the one hand, and the theory developed, on the other; and you can see the sorts of variations which the theory predicts, depending on the polarization. You can see the type of variation and experiment has been gotten in the radar measurement, and we would like to perhaps emphasize among other things we think there is value in the theory of explaining and guiding the experimental program. This, perhaps, -- now again, the idea of using electromagnetic scattering or ignition data to determine surface characteristics leads us to another point. We feel that work should be done using artificial surfaces.

In other words, if we can take and simulate here on the Earth the lunar surface, or whatever it may be, as best we can determine it from all available astronomical data or otherwise, we can then attempt to make measurements to see whether or not these measured values correlate, first of all, with the experimental values determined from radar and with the best known theories.

Now, what is the surface of the moon? As has been pointed out already, this is a controversial question within itself. There is data available, and I will show a series of three slides now which indicate about three possibilities, and the idea would be to simulate these here on the Earth, make measurements, and then try to correlate a theory for this surface with an experiment on this control

surface with another experiment made on the actual moon itself, from Earth-borne or space-borne radar, let's say.

Now, what we've shown here as the first possible type of surface based on the astronomical theory is a fairly uniform distribution of particle sizes with dense packing.

In other words, fairly dense packing of these particles. The larger ones filled in quite closely by the smaller ones in between.

A second type shown in the next slide where we have a preponderance of very large and very small particles. In other words, we have boulders and dust, and this is about it.

And No. 3 case, which again crops up in our best known astronomical theories, preponderance of very large particles with a little bit of dust, but it gets pretty well down to the bottom of the astronomical figure anyway. So, what we are saying is these surfaces which fit the theories proposed by the astronomers and others as to what the moon should look like could be simulated quite well here on the Earth and accurately measured to determine if the scattering properties or emission properties as a function of the type of surface structure, which is illustrated; then an attempt should be made to correlate that theoretical and experimental data with the other.

Now, going on from here, other things which we are interested in are spacecraft tests, of course, and not -- I don't want to get too much into the question at the moment of things like ion plasma and re-entry questions and what not, which we are interested in, and have done some experiments with, but more questions like the correlation of optical, active radar in radiometric photographs.

For example, we have made some effort in the past at taking air-borne -- these are fairly high-flying aircraft now -- radar photographs or radiometric photographs and trying to correlate and analyze these in terms of the theory developed by Dr. Peake and others at OSU.

Now, this we think should then be extended into the spacecraft tests which have been mentioned briefly, and along with this, of course, other experiments which we would like to see performed by the artificial surface studies; the control polarization from actual moon radar; and perhaps some work done in determining how to get an absolute radar calibration or the shot either around the Earth or planet.

Well, I don't know. I have tried to give a picture of our interests which essentially, as I say, boil down to being interested in correlating electromagnetic physical properties of various types of surfaces, especially those that are rough, since that is what we have to deal with most of the time anyway.

DR. PICKERING: Thank you.

[Discussion off the record.]

DR. PICKERING: We will have a ten-minute break.

[Short recess taken.]

MR. BROWN: I guess we are ready to go again from Stanford. Dr. Pickering had to leave, so I will introduce some of these people.

Dr. Waterman will present some of the material that Stanford is interested in, and Dr. Eshleman will present another facet of it, so I will turn the session over to Dr. Waterman.

DR. ALAN T. WATERMAN, JR.: Well, we will try to keep this subdivision in two categories from lengthening the proceedings here at all.

This comes about partly because of some fairly specific plans with regard to moon reflections which we had had some discussion with in connection with JPL and NASA people before, and some other more general work that has been going on for some time, and some broad future plans which Dr. Eshleman will talk about.

All of these fall into the general category of Earth-based studies designed to examine surface characteristics of, in our case, the moon primarily. In general, extending to planets and other material out in space.

This is all a direct measurement program; not a simulation study, and therefore should complement the discussion you just heard in the last two presentations.

Now, the first one of these -- the one I intend to talk about -- is designed primarily to take advantage of some high resolution in angle which is afforded by a large array which Dr. Bracewell had used for studies of the sun; the sun being approximately the same angular dimensions as the moon. This is also applicable for studies of the moon. This is an array used as S band, 3 kmc, with an apperture of 375 feet so that the beam width is roughly three minutes of arc, or roughly a tenth of the diameter of either the sun or the moon.

The intent was, then, to make use of this high resolution in angle in studying the reflected properties of the moon surface.

Now, to supplement the -- using that on the receiving end, the proposal was to use a high-powered CW transmission to, in effect, illuminate the surface of the moon. In its simplest form this would just be a literally pure CW signal, and it would be transmitted from a 30-foot dish so that it would illuminate the surface of the moon roughly uniformly. There wouldn't be any attempt to use a larger structure which might lay down a smaller beam width on the moon and one would then have the problem of keeping

the transmitted beam tracked with what the receiving beam is looking at; so the idea is we illuminate the moon with a beam wide enough to light up the whole surface and then examine with the receiving antenna.

The picture, then, would be roughly as follows: the moon's disk appearing here. The moon would be illuminated with a purely CW signal, and because of the libration there would then be differential Doppler shift in going across the face of the moon, as has been ably taken advantage of by the people at Lincoln Laboratories.

Now, if we used a transmission of 10 kilowatts CW with a stable system so that we could go down to band width of less than a cycle, we have got roughly a range of 50 cycles in Doppler owing to the moon's libration; so we can then divide the moon up into bins of equal -- bins corresponding to various Doppler shifts, the width of these depending upon the total stability of the system; and this could be -- well, such that it would be -- one could get several hundred or a thousand or so bins across the face of the moon that way.

If the receiving antenna array -- let's see, the receiving antenna array is a cross consisting of 10-foot dishes arranged 16 in one row and then 16 in a row across that way. If just one arm of the array is used a fan beam is obtained, and particular, the fan beam could be obtained

so as to illuminate a strip of the moon this way. The intersection of these two -- that is of this, then, given by the Doppler shift, and this given by the angular resolution of the arrays determines an area on the moon from which the return is coming, and if on the receiving end one has a variety of I.F. displaced slightly in frequency from each other, in a sense, then the various areas can be looked at simultaneously across one strip of the moon here.

This, you will notice, does not have the ambiguity that some of the work that has been already completed has had, which comes about because of finding the intersection between constant range lines and constant Doppler shift lines.

This also, of course, has application to detecting planetary spins, since, if we illuminate the whole surface of something and look at -- if we are talking about planets, then these beam widths are not sufficient to resolve parts of the planets themselves in angle, but we can determine whether a planet is rotating or not, again by the Doppler shift.

Now, an extension of this method involves getting some range information in addition. Now, basically we have in mind keeping the same CW technique, except this isn't a pure CW now. It is a frequency which, perhaps,

could be saw to over some given range, and then the cycle repeated; and basic philosophy here is that if -- this is frequency versus time -- if we are in some instance of time here now -- this is the frequency which the transmitter has at this instant -- at some previous time it had a different frequency which was transmitted, went out to the moon, and came back; so this frequency here, which left at an earlier time interval, corresponds to a transmission which is reflected from some parts of the moon surface having a certain range given by this delay.

If part of the currents local transmitted frequency is bled off to use as a local oscillator, then this difference here becomes the I.F., and that difference for any given range stays constant with time as we go along. So this effectively -- as far as the receiver is concerned -- is receiving a CW signal, and the I.F. band width can be narrowed down sufficiently to receive that CW signal, and we are getting our range information from this delay, or from this frequency displacement.

If we wanted to look at a different range, then we use the same -- we use a slightly different I.F. here. I.F. No. 1, and I.F. No. 2 then gives us two different ranges to look at at the same time. This can be done -- can be extended, of course, to stack up a whole bunch of I.F.'s and look at a whole variety of ranges.

Now, one apparent shortcoming of this technique is that it makes use of frequency change to give range information and therefore it inherently confuses range information with Doppler shift information. This, of course, can be unscrambled if you weren't to look at any one part of a solid -- known object like the moon, because the pure CW technique can be used to pin down the Doppler over the libration and then this -- the true Doppler, then, can be subtracted out of this range area.

To look at it another way, without any libration on the moon a ring of constant range is projected circle. The Doppler shift would then give up a return which -- if we were looking at the entire face of the moon -- would not come just from this ring but would come from a distorted ring which would be distorted out on one side along the moon's axis of libration there would be no shift, no displacement, and then there would be an opposite distortion on the other side.

So all this does is shift the -- distort the range rings slightly, and that can be corrected.

Also now if this is used in conjunction with this receiving array which is pinned down, limited area on the moon surface, then this technique gives us the ring of range and we find the intersection of that with this -- an area that is determined by the angular resolution of the receiving

antenna itself. The range resolution, then, achievable with this type of technique is probably going to be good enough so that a fine structure and range can be distinguished within the area covered by the receiving antenna beam width.

Well, there are a variety of elaborations which I could go on with, but I won't. I will stop here and turn the next part over to Dr. Eshleman.

A VOICE: Alan, how much did you say the Doppler range shift was?

DR. WATERMAN: About 50 cycles, I guess.

DR. VON R. ESHLEMAN: I have a short outline of what I am going to say. I am talking about a different program at Stanford University and the Stanford Research Institute, which is a natural extension of work that has been going on in the past which some of us are beginning to call the field of radar astronomy, all ground-based radar on astronomical bodies, and the program is a joint one using joint facilities, I would say, of Stanford University and the Stanford Research Institute.

Our past work at the University has included work on meteors, the aurora, and we are currently attempting to measure the interplanetary gap density between the Earth and the moon by using radio reflections from the moon. We are using the moon as a passive reflector in this case, not studying any characteristic of the moon, but just as a means

of getting the radio signals back to the Earth so that we can study the effects on the medium on transmission.

This is different from the Faraday rotation method, which you may be acquainted with, or maybe the high rotation within Earth's upper atmosphere. The technique we are using depends on dispersion in an ionized gas. It is independent of magnetic field so it measures the total ionization density between the Earth and the moon, and if the interplanetary density is more than some tens or hundreds of electrons per cc there is some hope that this can be measured by radar. And also in the past we have obtained very weak radar echoes from the sun.

The work at the Stanford Research Institute includes more work on meteor trails and aurora. It's also been studying at a hundred megacycles and four hundred megacycles radio obstructions from the moon, or using spaced receivers some attempt is made at surface properties, characteristics, and so forth; and also measuring the electron density in the Earth's upper atmosphere by the so-called Faraday rotation of polarization method wherein the simultaneous presence of the Earth's magnetic field and the electrons causes the polarization of electromagnetic waves to change.

At the present time we are building new facilities, joint facilities, that include a hundred and fifty-foot

spherical parabolic dish at a cost of about two hundred and fifty thousand dollars. SRI has built one of these already in Scotland. Another is about half completed at the University field site, and another one will be built for the sponsor of the Air Force Cambridge Research Center, and there is some talk of a few more also.

We are just also finishing the installation of a transmitter in the lower VHF band at 20 to 60 megacycles. It has 300 kilowatts as an average power. The Continental Electronics Company built this. It is about a million and a half dollar item, I guess. Also building a 1200-foot long array of log periodic antennas which would be used with this 20 to 60 megacycle transmitter primarily for continued measurements of the interplanetary gas density between the Earth and the moon, and also for attempts at obtaining sun echoes of a little more magnitude than were obtained before.

Other equipment that already exists at this field station includes various HF and VHF high power transmitters; two 60-foot dishes; a 2000-foot long array of yagi antennas; and about 28 acres of rhombic antennas. That is what we have now, and what is being installed at the present time.

In addition, there are proposals before the sponsor of the Air Force Cambridge Research Center from the Stanford Research Institute for a 10 megawatt peak power, 600 kilowatt average power, transmitter at 400 megacycles. This is one

that was being used -- transmitter. It's one of the early ones, RCA triode-type transmitters -- to be used with the hundred and fifty-foot dish.

Also using tubes developed -- Klystron tubes developed for the linear accelerators at Stanford. There is a proposal now before the sponsor to get 200 megawatts of peak power using eight tubes of the Klystron-type used in the accelerator. Here you have about 200 kilowatt average power -- at 3000 megacycles, and these same tubes work at somewhat lower peak power for longer pulses. At the 200 megawatt level the pulse width would be 10 microseconds. For one millisecond pulse they would have about 16 megawatt peak power output.

We are not sure whether the hundred and fifty-foot antenna that is being built will work well at 3000 megacycles, but it is hoped that it will give appreciable gain at this level. In the back of these things I have passed out there are some charts showing -- the first one, Figure 1, is a map of echo power on the orbit distance on the axis or round-trip delay time on the axis with the slanting straight lines indicating what the planets would look like if they reflected similarly to the moon, and they are spread, of course, because of the change in distance.

For instance, with the 16 megawatt peak power one millisecond transmitter at 3000 megacycles at a hundred and

fifty-foot dish we have a horizontal line that indicates unity signal-to-noise ratio without integration, which would show that we can see Venus over much of its orbit and detect Mars and Mercury as well. With the integration it would be expected that you might be able to see many other planets at lower than unity signal-to-noise ratio. Well, these new equipments would be primarily aimed at the hard targets, dividing the field of radar astronomy into the use of high frequencies, large antennas, and so forth, where the target is a surface of a planet or asteroid, or what have you.

In addition, we are very interested in another area of investigation wherein we are forced to use relatively low frequencies to measure what is being called soft targets -- that is, ionized regions such as the sun, the interplanetary gas density, the atmospheres and ionospheres of the planets and so forth. Here you are forced to use, as I say, low frequencies because only these are affected by the ionized region.

In order to do this we would like -- there is a proposal now being prepared. Incidentally, I see a sentence here in the last letter I got from these people who are interested in supporting any of this ground-based program, but at least I am putting out what our plans are, in this area in any event.

There is a design study going on now that is nearly

completed for an eight hundred-foot spherical parabolic dish which would cost about ten percent of the Navy's six hundred foot dish, a difference in price being due to the fact that no attempt would be made to make this of such a fine surface characteristic that it would work above 100 megacycles. Quite sloppy construction in that sense. Yet for this kind of investigation where we are talking about the sun, the interplanetary gas, and so forth, it is necessary to use a low frequency, and we need very high antenna gain and large powers to do this kind of thing.

On the second figure is indicated what might be done below 100 megacycles with an eight hundred-foot dish and a 10 megawatt transmitter. Again, at the 100 megacycle figure, for instance, you can see Venus as it goes behind the sun, as an example, and Mars over much of its orbit, and Mercury over much of its, and perhaps detect Jupiter. The sun, which is a very special target, at least at 25 megacycles, would have, with this system, 10 megawatts of average power, and an eight hundred-foot dish would be quite -- it would be at least 20 degrees above the noise.

There are all kinds of reasons I think why we should view the ground-based radar program of the sun, moon, planets, and so forth, as a logical adjunct to probing into space with material probes. This is just another way of looking at these bodies, using electromagnetic probes instead

of material probes.

I think I figured out once that the cost to get mass into orbit with a radio was about the same as the cost of putting that many grams of mass into a deep space probe.

MR. BROWN: Dr. Eshleman, do you think that using a spacecraft to help measuring the density might be of assistance rather than --

DR. ESHLEMAN: That would be for the gas density where you would be working down on a one-way path area; for extremely simple modulation techniques to be continued between the probe and the Earth. Something you would do on the one-watt or ten-watt level. A simple modulation. It is, I believe, necessary to be in a -- well, below 100 megacycles to do this job.

MR. BROWN: Thank you very much.

Any questions?

A VOICE: You may have come into this before I came into the room, so I apologize. Would you mention what integration times you were thinking of here in your developing your pulse radars?

DR. ESHLEMAN: Well, the lines are drawn with no integration, but --

A VOICE: Possible sensitivity?

DR. ESHLEMAN: There are all kinds of things you can say about this, and since people get as much as 50 db --

I don't want to say too much about that, but just for detection it looks like you can go essentially to the last planet in the solar system or something like this for integration, but you couldn't do any useful measurement.

A VOICE: How much time ~~drift~~ -- and how long a period do you think you are integrating over?

DR. ESHLEMAN: Compared to which system? All kinds of questions we have to answer. Let me say that integration was about 50 db, which was a 16-minute period. What did you use on the Venus, about 30 db?

A VOICE: Eight thousand pulses. About 30 db.

DR. ESHLEMAN: 30 db --

A VOICE: Closer to 40.

DR. ESHLEMAN: It is so many things that come into this that I am just using the 50 as something to throw out.

MR. BROWN: Any other questions?

I have a request to make on the questions. If the person who is asking the question would identify himself, and where he is from, it would help in keeping the records of what is going on.

DR. A. E. LILLEY: Dr. Lilley, Harvard.

Referring to the first presentation, what is the cell size based upon the bills cross approach and the Doppler resolution?

MR. BROWN: This is a question directed to Dr. Waterman? What cell size?

DR. LILLEY: Yes.

DR. WATERMAN: The angular -- I should put some numbers. The angular that I mentioned is about three minutes of arc here, so if it is this one you are talking about this is three minutes arc, and this is the other way around here. This is what is -- that's the three minutes of arc, and this one now depends on what sort of a stability you are talking about in terms of frequency stability and Doppler shift here.

DR. LILLEY: You mentioned one cycle, so that would be roughly a fiftieth of the diameter of the moon there?

DR. WATERMAN: Yes, but you could -- one cycle could probably exceed quite a bit.

DR. LILLEY: Thank you.

DR. WATERMAN: So this would be roughly a division this way, and this could be in the hundredths the other way.

MR. BROWN: Any other questions? If not, we will go on to the next speaker who will be Dr. Pettengill from Lincoln Lab.

DR. GORDON H. PETTENGILL: Thank you. In the interest of time -- obviously we are not going to be able to go into great detail in the experiments -- I'd like to just summarize them.

I will do this first, and then following that Dr. Green will give a brief outline of our plans. Probably the plans are of as much interest in a group like this as the experiments which have been performed.

Well, our interests in this field are sort of led into it, you might say, from other areas which have been in the past more prime responsibility of Lincoln. We started out being interested in the moon as a tool to explore some radio propagation, and some of the very first work that was done there -- is the Fricker and others -- were aimed primarily at observing the effect of the auroral conditions on the passage of electromagnetic waves through it, using the moon as a target, and a by-static geometry.

This more or less is a by-product turned up some confirmation of previous work on the properties of the moon as a scatterer.

They showed that something like a third of the projected disk was effective. This was, of course, the result of analyzing the frequency spread of the return signal. Also measurements -- and the correlation of this, I should add, with the libration, which could be predicted, was also confirmed.

Following that, the next work in this area was carried out at the Millstone Radar. Of course the first experiment there which came very close to this area was the

Venus work in February, 1958. Here again I should point out that Millstone was built primarily for other purposes than radar astronomy, and in fact its use in Venus itself was a very late addition to the experimental program there.

I think the most important result of the very first Venus experiment done there was the determination of the astronomical unit. As you know, it was a very marginal experiment signal-to-noise-wise. We are having to carry out a great deal of integration, sweep integration, in order to see any sort of an echo whatsoever, and very little can be said about the reflecting properties of the target, and the most important result being, we hope, a determination of the astronomical unit.

The experiment was attempted again in August and September of 1959. Unfortunately we were not able to achieve the additional capability, mostly in the transmitter, that we hoped to be able to, and as a result of this, plus other unexpected changes in the apparent target cross section, we were unable to complete the experiment at that time. However, as addendum here, interest to note that General Bane did get echoes at that time in cross sections that they measured a year and a half after our first work in '58. They were not particularly out of line with the negative results obtained at Millstone.

Finally, the most recent work, perhaps, that has

direct bearing upon this meeting today, are the lunar measurements made within the last year at Millstone Radar. Perhaps I can launch into this best by showing the first slide.

Our approach has really been of two sorts. The first is an attempt to determine the power distribution in range. This is a statistical approach, and coincides very closely with much work which has been done over the previous ten years. In this particular experiment short pulses were sent out 500 microseconds. The return signals were then integrated in much the same manner as the Venus echoes had been over a rather long period of time; some 16 minutes in this particular slide.

At any rate, the long integration time allows a great deal of spooning to be carried out, and by virtue of this we hope to be able to say something about the scattering law at the longer ranges, the ranges corresponding to the regions on the limb.

The spherical geometry of the moon makes it quite easy to interpret the power time of delay or range directly in terms of the angle, mean angle, with respect to the surface.

You can see if you think of the radar energy as being incident in this direction -- we are standing to one side now -- the angle at which this energy strikes the mean

surface has a 1-to-1 correspondence with the delay of that, with respect to some reference point, taking here the leading edge of the moon; and it is very simple to take the return power and plot it now instead of the function of range, which is a little more usual, as a function of the angle of incidence with respect to the normal.

In fact I plotted here the return signal intensity on a logarithm basis with a log or a cosine of that angle of incidence. This makes it quite easy to determine the cosine power exponent and thereby to distinguish between some of the possible scattering laws, both in the initial region here, and in the later region.

Now, on this plot you will notice what appears to be two rather distinct regimes. An early one, which we have called, loosely speaking, the specular region, because of its very rapid decay with range, or with range well away from normal, followed about two milliseconds back by a much more gradual decay. As you can see here from these lines this is the cosine-squared law here, and this is the cosine first-powered law, and just for purposes of comparison this initial slope here is very nearly a cosine to the 30th power. A very, very rapid decay.

I believe that this dependence of approximately 30th power in cosine can be related back to a mean echo of waviness, if you will, in this central region, and John Evans

has carried out some very careful compilations of work from many, many sources, finds that a very large amount of degrees, and yields something like five degrees, I believe.

If there are any questions you can direct these to him later. As to the sort of wavy structure of the moon, which lies under the nearly normal portion here, at the central region of the disk. Well, this deals some information on the bulk scattering properties of the moon. Now, in relation to a statement made by an earlier speaker, I point out that this Lambert dependence here was obtained using a circular transmitted polarization in one sense and circular C polarization in the opposite sense. If one were to use linear polarization I strongly suspect one would tend to develop a very random process of scattering that is going on here. However, even if one were to use linear polarization, the geometry of the moon is such that one would not be able to isolate which component of polarization was associated with any given region here directly, using this rather bulk approach where you are looking at rings extending completely around the disk of the moon.

Perhaps one final point here, you notice a slight departure from the true Lambert Law of the lunar limb. This would imply that there is also another mechanism operating here which perhaps is a little closer to the Lommel Zeeliger case indicating there are some rather

steeply inclined portions of the lunar surface, albeit very few of them, and these become important out near the rim where again the Lommel Zeeliger type dependence is very much stronger. There is no reason why we must have simple type of scattering in every region.

Well, to summarize here, if one carries out integration over here you find that something more than half the power is contained in this initial spike here; something less than half, then, is left for the Lambert region. This is when you carry out the mobilization directivity to assume certain constancy in the reflector properties of the moon which may not be justified, but if you do find that you have somewhere between 10 and 20 percent of the surface contributing to the Lambert scattering in the detail shown here.

Of course you must remember that this five-degree mean inclination which this rapid slope near the beginning indicates also means that you are sampling a very small region of the lunar disk for the specular component here; something very, very small indeed, and due to the limited libration the moon carries out you are never going to be able to see very much more in a region something like 60 degrees diameter on the surface of the moon; so that it may not be justified to claim this same specular behavior would apply to any region of the moon.

Well, one would like, of course, to pin down reflector properties of the moon a little more specifically to associate them with certain portions of the surface without having to go on very narrow beam widths. One way to do this has already been mentioned. This is to take advantage of the possible frequency resolution which is available nowadays, and due to the connection between the motion of the moon and the Doppler shift which is imparted, one can carry out an experiment, which I am sure most of you are already familiar with, whereby one breaks off surface of the disk, as seen from the Earth, into narrow regions which lie parallel to the apparent axis libration, as seen from the Earth, and by measuring these along with the range simultaneously one can hope to associate the power density observed with rather small regions on the lunar surface.

As pointed out earlier, to do that would be a two-fold ambiguity here. There are some possible ways to get around this. The scheme mentioned using a fan movement is a very excellent one.

Well, the next slide shows one of our results here. Again, I am sure many have seen this.

Here we have plotted the Doppler frequency in cycles per second where this is one cycle interval here. You can see that the total extent over which echoes are

being received is something like 11 cycles per second.

Why, this is the limb-to-limb change in the Doppler shift resulting from the maximum rate at which the moon can change its libration. In this dimension we have a range so that we are looking here now at a plot of the received energy as a function of the range back from the leading edge center of the disc, as we observe it, and the frequency, the Doppler frequency, which represents now a series of lines at -- well, parallel to the axis of libration here; and on the left we have a scale factor which is the relative measure of the received power.

The points to be made are rather simple here. You can see the outline of the lunar disc. This is a projection in which one can see these. You will also note that there is a great deal more power out here at the wings where the circles are -- perhaps I better draw that a little -- We have here a map showing the central region; head-arm disk of the moon, as we would see it, where the contours are circular, where these frequency contours now are straight lines parallel to the rotation.

If we take a projection from the side one can see that this will go over into a hemisphere now. The range rings lead from the side become very nearly straight lines, and of course the frequency intervals are still straight lines.

So that thinking of it in terms of this view from the side, rather than head-on, one can immediately interpret the plot that you see there showing the actual results as range in this dimension, and Doppler directly in this dimension.

One would then expect, of course, to see actual trouble within this region corresponding to some portion of the lunar surface. One also notes that if the reflectivity of the surface is more or less constant along any given range line, as it should be within the constraints of local variations and terrain, one will have a great deal more differential area contained within a given range ring and a given frequency interval here where these two constants are nearly parallel than where they are intersecting at right angles and where the common area is very much smaller; so one would expect to see more signal on the average from these regions than from these regions. Of course these regions correspond to the outer limits here, the high frequencies at a given range; and these regions correspond to the central section here, zero Doppler; so if we look again at the slide we will see that as we move back from the nose there is a great deal more signal at the wings here corresponding to these at the center.

Well, these are qualitative statements. One would, of course, like to carry this beyond and actually

map out any given small region of power density here on to a map of the moon and compare it with the optical situation. This we are now in the middle of doing. It is a little bit messy because of the two-fold ambiguity mentioned here. You have to take many observations. You have to separate out into the two hemispheres each of these observed reflections that you see. This is not very easy to do.

Well, let's see. I think, finally, just mention that this is the sort of technique which does not require a very high resolution in angle; can obviously be extended, we hope, once we have sufficient signal noise ratio to more distant reflection objects. Although we know a great deal about the moon -- we know its rotational rate; we know what the surface looks like very well; these statements are not quite so true as far as Venus goes, and more distant planets as well; so that we hope, perhaps, by applying these techniques to learn something about the rotational rate and the surface scattering properties of these planets.

As you can see, the range interval from here to here is a direct measure of the planetary radius, and the frequency interval from here to here is a measure of the product of the planetary radius and its rotational rate. So that we have, at least within some measure of accuracy here, which I may not be very good at this particular approach; nevertheless, we do have an estimate which can

be applied very unambiguously of the depth of the target, its radius, and its rotational rate.

I think now I'd like to turn over the discussion to Paul Green who will discuss some of our efforts which are under way now at another site and the sort of things we hope to learn from these.

MR. BROWN: Thank you. That product of radius in rotational rate, that is the project -- if the axis is tilted with respect to you it would be projected?

DR. PETTENGILL: That's right.

MR. PAUL E. GREEN, JR.: Well, I just want to take a couple of minutes to tell you some of the facilities that we are building at Lincoln with which to carry out experiments of the type that have just been discussed by Dr. Pettengill and some others.

As you can see, we and everybody else who is doing Earth bound radar experiments on the planet are basically interested in three things. We are interested in distance measurements, and we are interested in surfaces, and we are interested in soft regions, as Dr. Eshleman put it, which could include -- say, the sun or ionospheres; and also interplanetary free electron densities.

Well, the facilities that we have in mind are contrived to cover this region of study as efficaciously as possible. Up to now we have been limited to one radar,

namely Millstone Hill, and we feel strongly that we need the highest possible sensitivity in very high frequencies and also down around the high HF or low VHF region. Accordingly, we have in the middle two facilities; one called Haystack Hill, which is a 120-foot dish in a ray dome supposed to be good to X band frequencies; and the other an installation that is nearing completion right now at El Campo, Texas, which operates in the 38 megacycle region, and I will just say a couple very brief words about each of these.

The Haystack Hill facility, which is based on our availability to us of a ray dome 150 feet in diameter of the type used for the Dimuse system. This is a space frame ray dome, so-called, that is, it's got metal struts. Model studies that we have made using a 15-foot scale model altimeter wave frequencies have indicated that the use of this space frame type of construction should lose us only something on the order of about 20 degrees K in a radar observation.

Accordingly, we think it will work, and we have gotten support from the Air Force to build within this ray dome a 120-foot diameter dish. The exact nature of this dish, the type of construction that it will be, is at present unknown since the particular contractor has not been selected as yet. The El Campo facility is built around

a piece of equipment that we have had kicking around for some years, namely a 600 KW -- CW Continental, the same company that has built the Stanford transmitter. The transmitter used for experiments done in the early days of ionospheric scatter, and this thing can be turned from 20 to 70 megacycles, and we have chosen to operate it at around 38 megacycles at which frequency we are building right now a phase array of antennas which will consist of eight rows of 128 dipoles each. These rows run in a north-south direction and dealing in an antenna beam is eight degrees wide in an east-west direction, and one degree wide in a north-south direction.

In order to change the angular orientation of this thing, which is to be adjustable from 30 degrees above the horizon up through zenith and out 30 degrees above the horizon in the other direction, if necessary; sort of a poor man's approach was taken of buying up huge quantities of coax, cable of different lengths, and every time we want to change the elevation of this thing some poor soul or souls have to go out and unplug massive quantities of these cables. Nevertheless, we feel that we can make solar observations, which is the main purpose of this instrument, for a couple of days at a time, and then change the antenna orientation even in the part of the year when the sun is moving most rapidly.

The gain of this antenna is about 35 db. The current status as of right now is that the transmitter is to be fired up within a couple of weeks after several years of being in mothballs. An attempt will be made on the sun, using the simple corner reflector antenna that sits there now and was used for the early ionospheric scatter experiments. This is not expected to be successful, but one reason we want to do it is to exercise the equipment and check our data processing arrangements.

Then probably round the end of July the phase array should be nearing completion. Right now the poles are being put in place and the aluminum elements are being assembled; and then we hope to go after the sun with an antenna that is really pointed toward it instead of working out of the side of a rather fat lobe as we will have to do shortly with the present antenna.

There is a third and a fourth antenna installation that we have in mind that I would like to just mention. One which I came off without the numbers on is a millimeter wave radar, and it is being built by the Lincoln Laboratory mostly for studies of the moon and the upper atmosphere. The idea here, I think, goes back, as much as anything else, to the notion that has been propounded by a number of people like -- Tom Gold is one -- that you want to make these surface studies -- that is, why the different range

in wave lengths -- you can, and we heard some comment earlier about how much was expected of this diversity in wave lengths on the terrestrial observations, and it doesn't seem to pay off to the extent that we hoped.

Nevertheless, we would like to arm ourselves with the widest possible arsenal of wave lengths to examine objects like the moon, so there is an eight millimeter radar under construction using, I think, something around 10 to 12-foot diameter dish -- very small -- a hundred watt CW power, and this rig is to be made mobile so that it can be put at a very high altitude, thus eliminating some fairly large proportions, which I forget, of the continuation effects that one has at those wave lengths due to the Earth's atmosphere.

I really don't recall what the status of this thing is. I would suppose that within something like a year we might be able to get this in operation. It is actually being built right now.

The fourth facility that I wanted to mention -- I sort of hesitated to discuss it since it is not too interesting -- is a 60-foot X band dish that is being built right now, and this is not tremendous, but it's a very new thing, a 60-foot X band dish, but there are constructional features in it that I thought you might find amusing. It is just possible that this might be the

technique to be used on the 120-foot dish, although we don't know.

If I could see the first of those little slides I will show you a picture of this dish. It doesn't look at all unusual when you see the slide, but on closer examination you find that the structural members of this thing are not made of solid material at all, but are made of this so-called honeycomb stuff that you can buy now. It is fine aluminum material extruded -- or rather sheet metal that has been fabricated into a structure like a honeycomb. This thing, which I hope you will see, was built by an airplane company, the Aeronca Corporation, and has a rather astonishing amount of rigidity, considering what its weight is.

Well, let me go on. If you can get the slides working we can show them. I don't know that they are too interesting to you, and maybe the best thing for me would be to ask that if anybody wants to look at these pictures come around and see me later.

So let me just conclude by mentioning what these different things are supposed to do for us.

We are interested, as we said, in these categories here. We are interested in distance measurements, observation of surfaces, particularly of the mapping type that Gordon Pettengill just described, and we are interested in beginning some observations of soft regions like the sun

and the planetary ionosphere which we haven't had a chance to start on yet.

One thing that the capability at two different frequencies should be able to buy you, and we feel this is very important, is the opportunity to cancel out the -- to measure and cancel out the effect of interplanetary electron content in making a range measurement. We hope to use El Campo on Venus next time it comes around, and if we are able to get a successful range measurement at 38 megacycles on Venus, and if at the same time we are able to do anything at another frequency for which we only have Millstone plus this 60-foot dish, which I am showing you pictures of, which is not expected to be very much more spherical, but comes out about the same, but if we were lucky and about to get range measurements simultaneously at these two frequencies then we can do -- and with the same accuracy -- we could conceptually, at any rate, solve for the total integrated ion content between the antennas and the target and say something about the interplanetary electron density. It is not expected that the electron density on Venus would be so high that it would show up as a large discrepancy over and above the total amount of electrons given by the Earth's ionosphere plus interplanetary space. It would take something on the order of a very deep atmosphere having no more than 10 to the 8th electrons per cc

to even show up as a wild departure from what one would expect. Nevertheless, it is an interesting series of experiments to do some time, and we hope that such a simultaneous two-frequency observation might just be possible; maybe even by the next path on Venus.

In the distant future these two-frequency techniques can be expected to be quite valuable for measuring the integrated electron content and then turning around and subtracting it out of the measurement so as to refine the distance measurement even more.

I guess that's really about all I have to say. As I said before, if anybody wants to see the pictures of this antenna made out of honeycombs, I have them there.

MR. BROWN: We had trouble with the equipment. I'm sorry, Paul.

Mr. Karl Linnes, JPL.

MR. KARL LINNES: Were your corrections for range measurement referring to your point of megacycle measurement?

MR. GREEN: The ones we might some day hope to do?

MR. LINNES: Well, were you referring to a particular frequency band?

MR. GREEN: Yes, what I was saying was that at 400 megacycle, for example, there is no particular retardation due to the interplanetary or ionospheric electrons. We figured that it was something like a part of

10 to the 6th in our Venus experiment of 400 megacycles, which was a much finer resolution, but 38 megacycles can be expected.

MR. LINNES: You are referring to the lower frequencies?

MR. GREEN: That is where you pick up the discrepancy, and then you would solve for the total integrated electron density, and if your 38 megacycle observation could be made at -- with a fine enough range resolution, you might then be able to improve also the original range resolution that you made at 400.

PROFESSOR K. M. SIEGEL: Professor K. M. Siegel, University of Michigan. Clarification about one comment that Pettengill made. When he points out that half the energy is specular return, I just want to point out that this statement; is signal-to-noise ratio dependent and is pulse life dependent. If you had a very poor signal-to-noise ratio, would have said all the energy was in spectral reflection. I just want to state that as I consider a clarification.

MR. GREEN: I don't think this particular noise ratio is much of a problem at the leading --

PROFESSOR SIEGEL: No, I didn't say it was a problem at all. I just said that you would have reached a different conclusion if you had a much lower

signal-to-noise ratio. The conclusions you reach --

MR. GREEN: As for the signal-to-noise ratio, I don't think anybody would make the mistake of observing that signal-to-noise ratio -- that 50 percent of the signal-to-noise ratio appeared in the first bit and then turn around and say that this was 50 percent of the signal. He would know that he had to cancel out the noise which might be large, but which wasn't in Pettengill's experiment.

PROFESSOR SIEGEL: You hope he is not?

MR. GREEN: Yes.

MR. BROWN: Yes, well, the conclusions you would draw would depend upon the method of analysis, perhaps, and whether the signal-to-noise ratio comes into this method or not; depends on what you are working with.

Dr. Von Eshleman?

DR. ESHLEMAN: I would just like to state that I think one of the most exciting things about this range Doppler resolution on the moon is that once you have the transmitter power, which is all technological to me, and do this on the planets you can hope eventually for what amounts to angular resolution, which is much better than -- can even be obtained optically. Even though the energy itself gets mixed up as it comes through our own atmosphere, when you're looking at range and Doppler, it doesn't get mixed up that much and you are actually looking at it -- things on a

planet which submit an angle, which is much less than the second of arc planet, for example. Of course we have to underline that all made has more power, but we hope --

MR. GREEN: You put it another way I thought was quite good; namely that it is independent of the distance.

DR. ESHLEMAN: Yes.

MR. GREEN: If you were observing Pluto just so you had enough power to carry off this tour de force you would be able to measure with the same resolution and of the surface as you could measure with some nearer body with less power.

MR. BROWN: Are there any further questions?

If not, I will turn the meeting over to Professor Siegel from the University of Michigan.

Professor Siegel?

PROFESSOR KEEVE M. SIEGEL: I don't have much of a voice ~~today~~, which is probably beneficial, except the time is running out, and I don't have slides which is probably also beneficial.

I frankly don't want to take any time summarizing work we have already published. I think that it is a waste of time; or maybe it isn't, but nevertheless we have enough stuff we haven't published that I do want to spend a little time on.

I want to point out some publications we have

coming out; some at journals; some with dates on them; some which haven't been sent to journals yet, but on work that already exists, and progress reports.

We have been able to show that the ratios of the permittivity of permeability and conductivity of permeability that we have previously gotten for scattering surfaces on the center of the moon are, in fact, good average values for the lunar surface itself; not just statements about scattering centers.

The second paper I will talk about is tektites with the help of John O'Keefe of NASA, and Henderson of the Smithsonian Institute. We have borrowed and measured permittivities and some permeabilities and some conductivities of tektites and this should be coming out in the "Astrophysics Journal" shortly.

With the help of Harold Urey we have selected the kind of meteorites that he felt would be materials like that found on the moon. He has made measurements of permittivity and permeability and conductivity for meteorites. With the help of several geologists, including a team from principally Polytechnical Institute, we have brought and made measurements on a large number of rocks that people expect to find on lunar surface. This is compared with other geologic data, most of the constants people are referring today; physical constants of different kinds and electro-

magnetic constants are being measured from these rocks. We make the electromagnetic measurements at Michigan, and Rosenholtz is making some of the other measurements at Polytechnical Institute including hardness and other things like that.

Also I want to make some statements about thermodynamic constants following not the work of Peake, but following the large Russian literature on the subject. We have derived some of the thermodynamic constants for the surface of the moon. The way we did this was we chose our values for the electromagnetic constants and then, by comparing the fix and variable components of the temperature for the lunar surface, you get the fix component presumably from radiation which occurs at the surface of the moon itself; you get the variable component for the reflected sunlight off the moon.

By making a comparison between the fixed and variable temperatures as a functional frequency, and using the data plus the electromagnetic constants, I think that we previously had published, we came up with values of the thermodynamic constants; and we found then, when we took these thermodynamic laws and wrote them down and stated what these particular laws would have given you as far as temperatures are concerned when you used radiometers and you look at the surface of the moon, we found, with the

exception of one point, which, if my memory was correct, was an X band; we found that the rest of the points all fed very well on this theoretical curve, and I think if I remember right this one point was a point of measurement about 175 degrees Kelvin by Trotsky, which did not fit the curve; but I think there are 15 or 20 other points which do fit the curve, and I want to thank Dr. Barrett from the University of Michigan for assembling us a bibliography analysis of what experiments have been made, because for people who do not work in the astronomy and radio astronomy field we found that we would have missed many of the measurements.

Well, I think I have summarized fairly well stuff we do on the moon. I want to talk about Venus, and I first want to make some comments about the kind of things you can learn from a spaceship.

By radar from a spaceship we can learn the growth surface features of the planet Venus. We can learn about the ratios of the permittivity of permeability and conductivity of permeability. We can learn about location of scattering centers on the surface and about spin rates. By making Faraday effect measurements we determine the magnetic field of Venus and its electron density. By making Luxemburg effect measurements we can learn the collision frequency, and by measuring the transmission and

reflection coefficients we can also determine the collision frequency on the electron density.

For example, if you had a transmitter on Earth and you had a satellite moving around Venus, you, of course, can work these out. You notice that I determine the same things by several different methods. I think this is important.

If we really learn the electromagnetic constants of Venus by use of passive data, from the I.R. to radio wave specter and by separating a fix from variable components, as well as line specter. By this analysis, analysis of temperature, we come up with thermodynamic constants such as the thermodynamic conductivity. By making measurement of the function of frequency we can determine the electron density and plasma frequency. By varying the modulation we can determine the collision frequency.

Now, I covered that part. I wanted to tell you about a crazy theory which I have about Venus, and which I am in the process of writing a small paper on. If I had been in Michigan the last few weeks it would have been typed, but I haven't been there.

The paper really is based on certain ideas about what happens when a planet gets closer to the sun, and I come up with -- maybe I ought to call them excuses -- but nevertheless I come up with physical conjectures that allow

the atmosphere of Venus to be more dense than most people believe, and I come up with physical reasons for the electron density to be extremely large; very, very much larger than the planet Earth; and I was quite skeptical about this theory until some time ago I heard about M.I.T.'s Venus experiments.

You see I thought there was a possibility, and still do, that if my other crazy theory happens to be right, then M.I.T. might be operating close to the plasma frequency of the planet Venus. This requires a very much higher electron density than ordinary theories give, but it happens to agree with the kinds of electron densities that my crazy theory seems to use.

Thus, this would save the small changes in the electron density. If M.I.T. were working at the plasma frequency, then it would turn out that the particular aspect, at that particular time, that they may have been just above the plasma frequency; another time they may have been just below it.

I have gone through this kind of thing as a function of ideas of radiation and the length of time. The temperature seems to remain constant as a function of rotation of Venus; several other things which would allow you to predict denser atmospheres at higher electron densities.

This last part of what I have just said I wish you would not take with the same conviction as the first part of my statements. This last part -- if you say it's conjecture or a crazy idea, that's O.K.

I want to point out that on the new publications I was referring about, the new work we are doing on the moon, this was done primarily for the United States Army, working with the Corps of Engineers, and previous work was done for NASA. All of the Venus work I am talking about has been done for NASA. We have been working with the Autometrics Corporation, and the rock work that I was referring to has been done under this contract as well as this variation between fixed and the variable component of temperature.

Concerning what we would like to do in the future associated with Venus, frankly we would like to design electromagnetic experiments to be put into spaceships, and we would like to furnish ideas, which we don't really object to if other people discard, but nevertheless we would like the opportunity to furnish ideas for other people to make measurements associated with ground equipment and just to check out some of our theories.

We would like to -- we plan to work closely with -- possibly in a corporate effort -- with the Space Sciences Laboratory at the University of California, which is headed by Professor Silver.

MR. BROWN: Are there any questions for Professor Siegel?

PROFESSOR MARSHALL COHEN: (Cornell) Could you give us a few words about your crazy theory?

PROFESSOR SIEGEL: Frankly, I'd rather send you a copy. It's based -- well, I don't want to -- I shouldn't get into it. I want to get into it, but I shouldn't get into it; I will put it that way. I'll send you a copy and anybody else who wants it.

A VOICE: Is this a preprint?

PROFESSOR SIEGEL: It will be a preprint because it should be typed.

N. G. ROMAN: (NASA) I have two questions.

First, your statement that variable component of lunar temperature is due to reflected sunlight bothers me, because I thought the main contributor to the variable component was the fact that the moon actually changes temperatures within the sun angle.

The second question I will ask at the same time, while it is unrelated, is if you could give us at least a sentence or two of the results of all these things that you were discussing in terms of tektite measurements and meteorite measurements?

PROFESSOR SIEGEL: Sure. All right. Your first statement is about temperatures. I can't distinguish the

two things you said.

When temperature changes, that's clearly a variable component. I thought it was clear that the sun has something to do with it. Whether it is reflected sunlight or not, that is something else; but I am comparing the variable component with the fixed component with the variable component becoming smaller and smaller as the wave length becomes larger and larger, and it is these kinds of laws that I was comparing, and using Trotsky's work in going backwards, and several other of the Russian theoreticians, one can come up with ratios between the thermodynamic conductivity and the skin depth, and the skin depth being dependent upon the electromagnetic constants in the wave length; and so going backwards that way one does come up with the thermodynamic conductivity, and then one can plot -- after he just uses two temperature points -- one can plot the complete temperature dependence. One has the complete temperature dependence. One then finds that you predicted all the other points and, in fact, you don't have to pick two points which are far apart, you see, because that would make it look like you were on the data. If you just pick a couple of points in the infra red you would have predicted the whole microwave behavior.

The other point I wanted to make -- well, the other question, I mean, I wanted to answer -- excuse me --

is the results. Well, the results on tektites was the fact that the permeability was low indeed, as a man whose name I will mispronounce -- Senfull -- had also shown, and he made some permeability measurements on a few tektites. The dielectric constant of relative permittivity was, in fact, quite high. We distinguished between tektites and Libyan glass, we hope, for all time; namely, the relative permittivity of Libyan glass was approximately 4.5. The relative permittivity of all tektites measured lay between 6 and 7.5. So, one found, by just this kind of measurement, that these kind of glasses no longer fit in with the tektites. We found that these relative permittivities, however, were too high with our then theory concerning just the specular regions of the moon, but now going further and coming up with a reflectivity on an average basis for the total in itself. The result in the average basis came up with the same kind of electromagnetic constants that we have previously had for the scattering centers.

The results on meteorites are much more interesting for the solid state, for instance, than one had a right to believe, namely the variation of permeability with frequency, is quite surprising indeed.

Of course it turned out that the constants that one got at higher frequencies were, of course, very high indeed, and that these could not fit in with the previous

values reflection coefficient.

If one takes a reflection coefficient and starts working it out, say from Lambert's Law story where most people would refer to reflection coefficient of the albedo, you would have noticed from Pettengill's curve that as he took the result, and if he went over to the axis for his Lambert's Law curve he would have, in fact, come up with the ratio between sigma zero.

Well, he would have come with his ratio between those two cross sections, and this ratio would be the -- most people would call it the albedo, but if you went through conservation of energy using the polarizability term in the dipole derivation of Lambert's Law you would have directly related it to the reflection coefficient that you, in fact, get in electromagnetics. You will find the number he got was in the order of magnitude of four times ten to the minus fourth, and let's just try to remember his curve, but you will also find from our published articles that his is the kind of reflection coefficient that people would have got at small pulse lengths. The Lambert's Law answer is pulse length independent, so as a result we would expect that result to agree with the answer we would have got in very small lengths, according to our theory, and as a result we found that the two reflection coefficients, in fact, do agree; in fact, agree in the first significant figure.

Then, working backwards, one can come up again with the dielectric constants of permeability and permittivity of ratios. The ratios are as we previously published them.

Did I answer all your questions?

N. G. ROMAN: Yes.

AFTERNOON SESSION

2:00 o'clock, p.m.

DR. H. L. RICHTER, JR.: I think it is about time to start the next session. We have a rather full program. We have a couple things added that are not on the printed program, so we will try and move along.

I would like to make a couple remarks to begin with.

[Discussion off the record.]

DR. RICHTER: We have a number of presentations to go through. The first is from Cal. Tech., and we have asked Mr. Bolton, who is, I guess, in charge of the radio astronomy activity down at campus, to say a few words about his interests.

John?

PROFESSOR JOHN G. BOLTON: My Institute operates a radio observatory in the Owens Valley, a natural trench in the Earth surface and pretty good shield from interference. It has been in operation with two 90-foot steerable antennas for some 18 months now.

During the first year we operated the antennas as individuals doing virtually a source-finding program on one of them and an introduction to 21 centimeter absorption spectra on the second one.

Since January we have had them in operation as an interferometric pair operating on 960 megacycles. The choice

of 960 was a fairly logical one. The local oscillator is at 960, so this is one frequency we did not receive.

We have fairly standard preamplifier crystal mixer-type receivers. The input temperature is about 300 degrees K. The primary beam of the antennas is around 45 minutes, and we have facilities of different base lines to antennas on the rail track for operating with fringe spacings between 30 minutes of arc and 2 minutes of arc.

Limit of sensitivity, using something like a 10-second time constant, is around 2.5 times tenths minus 27 watts per square meter of cycle per second.

A reasonable operating limit of sensitivity, let us say, where you can see something every time you look at it, is about 4 times 27 units.

Our program in general can be described as what and why of the radial stars, and our investigations are directed towards the precise positioning of these objects, the measurement of their angular diameter, the measurement of their spectrum, -- that is, frequency versus intensity in the measurement of their polarization -- and anything else that comes up.

During the last six months, or five months since the interferometer has been running we have measured about 70 positions with a fair degree of precision. Thirty of these have resulted in identifications with galaxies.

We have measured angular diameters of 280 of them, 130 galactic and about 150 extra-galactic. We have looked for polarization. We have done a certain amount of spectrum work.

Now, probably two of the most interesting observations, contributions, we have made, are in the very near reaches of the solar system on the planet Jupiter, and in the most distant reaches of the universe with an object which I will call 3C295.

Now, 3C295 -- the number refers to the third "Cambridge Catalogue of Radio Stars" -- was known from measurements by the Manchester Group of radio astronomers to have a rather small diameter, less than some 10 seconds of arc.

The Cygnus A radio star is the collision of galaxies that is 10 to the 8th parsecs, and its angular diameter measured radio-wise is about 80 seconds of arc. So that presuming these things are on the same scale it is suggested that this 3C295 was at least eight times as far away.

Its intensity, however, was knocked down by a factor of 64 to Cygnus, so it would seem that it was intrinsically rather luminous. We got a very precise position of this and Rile Ellsmore at Cambridge had also a precise position, and these two agreed within some three

seconds of arc.

Visible on the Schmidt plates, or barely visible on the Schmidt plates, were two extremely faint blotches.

When Minkowski shot this field with a 200-inch telescope a cluster of galaxies, about 15 members within a minimum arc, showed up on the plates. There appeared to be two possibilities for the radio source. We could not distinguish between a large number of galaxies within the arc. One was luminous system, and the other a system with a somewhat peculiar color. Minkowski got spectrum placing these two both within split -- slit of the telescope, and in four and a half hours the oxygen two lines, which are normally at 2727 angstroms, showed up at 5500. The object has a velocity .46 of the velocity of light. This is two and a half times the furthest object measured so far, and vindicates the promise of radio astronomy as a selection to -- for getting further out into the universe.

The other particularly interesting object is the planet Jupiter. The history of this is with Russell Slurmacher of NRL. He is doing preliminary work with the new NRLH 4-foot dish at 10 centimeters; measures an unusually high temperature of 10 centimeters for Jupiter. I think something on the order of 500 degrees. Previous measurements by Mayer of 3 centimeters gives a value of about 150 degrees I.A. watt one would expect from the infrared measurements.

Now, seems to be something peculiar about this so we looked at 900 megacycles and found, in effect, a temperature of around 6,000 degrees, pretty much the same time the Green Bay people looked at 400 megacycles and 1400 megacycles, and the spectrum seems to be more or less independent of wave lengths.

As one of the final measurements on our angular size program I had hoped to measure the diameter of this thing, expecting it to be around the 40 seconds of arc of Jupiter. In the meantime Christianson, and Roberts have been doing polarization experiments on the Crab Nebula and MH7 galaxy from which we see synchrotron radiation and from which the optical light is linearly polarized.

Now, those measurements didn't produce anything, but just about the same time we were gradually moving the antennas out along the baseline and we looked at Jupiter for polarization, and 30 percent linear polarization parallel to the equator of the planet showed up, and I can roughly sketch our measurements since then, plotting here the amplitude of the interferometer fringes against the base line at 960. This wave length of almost another foot. We go out here to 1600 wave lengths. 400. 800. We place 1600 out here. Defectively zero spacing or 200 Lambert. The polarization parallels the equator; gives us points up here

perpendicular to the equator; points down here; and using the cross-polarized horns on the dishes to two dishes to detect just a linear component you have a point down here. Out 400 feet nothing much happened, and then in the range between 400 and 800 feet this suddenly went down, this suddenly went down, and this more or less stayed constant.

This corresponds to a fringe spacing of roughly 4 minutes of arc, and these linear polarized components result in the arc of this.

Going on further from there, this curve gradually goes down to 1600 feet. I might say about this point we get into difficulty measuring the planet. One-tenth of Jupiter is about our sensitivity limit, and sketchy points down here, and this, if one can believe it in noise, does something like this again, and the best guess that we can make at the distribution of radiation which corresponds to -- in here is the planet's diameter. The linear polarization is -- looks probably something like that. That is, the majority of it is effectively out at between three and a half and four and a half times the radius at the planet.

The unpolarized component has a distribution which is something of that order. A certain amount of theorizing is being done on this by George Field of Princeton, and Everett Davis, a student at Cal. Tech., and we believe that we require a polar field of something on

the order of ten galvs to account for the mechanism on synchrotron radiation.

Now, one of the difficulties of working out a really detailed model is that we don't know how big the north-south extension is; whether it is in a distinct equatorial belt or whether one actually has something more spherical in shape. All we can say is -- I might say that in taking these -- these are our individual stations. Eight hundred, sixteen hundred, four, two, one hundred, and in order to get separations intermediate between those we observe either east or west of the meridian. The dish is looking over like that and the base line is a perpendicular distance. This, of course, involved some rotation of the planet, and from the scatter of these points down here I think we can definitely say that the north-south extension is certainly no bigger than the east-west extension, but we can't put a limit on anything further than that.

I think my fifteen minutes are about up.

DR. RICHTER: Are there any questions?

Thank you.

Listed next on the program are comments from Cornell. I guess Dr. Gold couldn't come, but Dr. Cohen is going to comment.

DR. MARSHALL COHEN: I am not as intimately connected with the radar program as some others, but I think

I can give you somewhat of a story and probably answer some questions, if there are any.

Cornell is beginning to set up an observatory, the Arecibo Radio Observatory in cooperation with the Air Force Cambridge Research Center under contract with ARPA. Arecibo is the fourth city in Puerto Rico and the radio station itself is going to be in the type of *esperanza* which is 15 or so miles away. This will be a radar of some 5 megawatts or so with a thousand-foot dish. 430 megacycles gives one-sixth of a degree beam. Pulse lengths to something like 10 milliseconds.

The reflector is going to be a section of a sphere fixed to the zenith that will be some chicken wire or hardware cloth material. Since it is a sphere the feed can move around on a different radii so the beam can be directed to different parts of the sky. Feed, of course, has to be corrected for spherical aberration. The plan is that the feed will be able to move so that the beam will be able to move, plus the 20 degrees off the zenith.

Location is in Puerto Rico in the tropics because we wanted to be able to see the ecliptic -- the planets and the sun and moon -- all who come directly overhead in due course, and without a great deal of zenith motion we can see all these things.

I have two slides that show a little bit of the

situation in Puerto Rico.

This is a view taken from the air of a limestone sink hole which is some 1500 feet in diameter and 500 feet deep. It is not exactly a sphere, but there is a very large part of a million yards of dirt that has already been removed from the hole that was needed.

It is mostly tropical jungle, and this is a tobacco farm down at the bottom. This is what it looks like now, although I think there is a road that goes into the place now; but otherwise it looks just like this.

Could I have the next slide, please?

Here is an artist's conception of what this thing may look like, or will look like -- pretty much about this. The mountains aren't shown quite right. The dish itself is a thousand feet in diameter. That is this business from here to here. The hills around pretty much make a 15-foot diameter hole, so there is quite a bit of shielding on the dish itself, and the iron feed which directs spherical aberration is this thing. The feed has to stay on a radius of the sphere within six inches at 430 megacycles. The feed is 96 feet long. The feed is supported by a truss mechanism so, so that it can move from here to here and thereby get the zenith motion of 20 degrees. This is essentially an elevation motion along here, and then this truck here, which is an arc of a circle there, that thing can spin around a

vertical axis. So the motion of that truck is on a circular ring, which is held on a triangular frame, which is held from a spider web, which you can see held from these three concrete towers. Turns out that the overhead spider web is much more rigid than a bridge would be. Well, this is the sort of thing that is presently being designed.

The feed which was shown near that long line of feed over the spot of the square wave back is a square because we want to get two polarizations. It will be tapered so that it has the correct phase velocity along it, and thus the rays will come out incorrect for the spherical aberration. The rays will come out with the right phase velocity. Present status is that the feed itself -- which I think is the trickiest part of the whole radar -- the feed is being designed by a technical research group in Cambridge, Massachusetts, or some place near Cambridge, -- is being designed and will be conducted by them; the reflector and the overhead support, the spider web thing, and all the towers and associated things like that, being designed by a joint group of engineering and architecture firms.

The plan is that this radar will be operating some time short of a year from now, in the summer of 1961. At least it will be ready for testing in the summer of 1961. First use of this machine will be as an ionosphere probe;

that is, we will be measuring electron densities as a function of height. Electron densities will be measured by getting the echo from the -- the scattered echo from the free electrons in the ionosphere. This is not the ordinary reflection-type echo that we get from the ionosphere. This is 430 megacycles, and this is the so-called incoherent scatter echo from the electrons, free electrons.

Strengthening of the echo will be given the number of electrons per volume, whatever the volume happens to be; that is determined by the pulse length; and if we can measure something of the spectrum of the echo that comes back, then we can tell by the Doppler shift what the temperature of the electron is. We can measure number and temperature as a function of height and time.

Possibly we can say something about the magnetic field. We will have polarization measurement facilities there, and it is not clear exactly how one gets magnetic field at each point anyway from the measurement, but I think there will be magnetic field information that will be there from the Faraday rotation of the echo as it goes up and down the signal.

So there is an ionosphere use, and this is going to be the first main use for the instrument, but then it obviously has a great deal of use as a planetary radar. The radar will have a sensitivity of about 43 db greater

than Millstone, and so we can begin to do on Venus what Millstone does so beautifully on the moon; that is, make one of these maps of a Doppler and range; the two axes being Doppler range.

It is really not quite that simple because the Venus is down by more than 40 db on the moon, but still we should get signal-to-noise ratios without integration of some 20 db, or something like that, when Venus is closest; so I think we will be able to say a great deal about the surface of Venus in the way that the early radar measurements on the moon could say something about the surface of the moon.

We'll certainly be able to say something about the surface of Mars from this, in the same way. Mars, of course, is a smaller target than Venus, so our signal strength would be a lot stronger on Mars. We will be able to say something about it. I think we would be able to get an echo, at least, from Jupiter; depends on how sophisticated and how much trouble we use with integration. We should be able to see Jupiter, at least, although it will be some years before Jupiter will be far enough north. Jupiter at the moment is in a southern hemisphere, and I think it's 1964 or '65 before it gets far enough north to be seen from Puerto Rico with this instrument.

Well, then, we would, of course, have a look for Mercury and the outer planets, too. I don't know really

whether we would be able to see the outer planets. I rather doubt it.

We would have plans also to look at what Professor Eshleman this morning called south targets; the sun in particular. The first story on the sun is that at 400 megacycles you get no echo because the wave goes all the way in and is absorbed completely before you get anywhere near the reflection, although that's certainly true; but on the other hand if there is a strong magnetic field in the corona you have two reflection levels, and the reflection level for the extraordinary wave is very much higher than that for the ordinary wave. We have made some calculations lately that show that at 400 megacycles above a large sun spot you might be able to get an optical depth of only something on the order of 2 at the reflection level, so I think it will be of great interest to look at active regions on the sun with this radar because if there is a lot of magnetic field on the corona we should be able to get an echo.

The calculation that we made, using a sort of reasonable model for the magnetic field above the sun spot, -- the calculation showed it would be a signal-to-noise ratio of 1 if we used the 700 cycle band width.

Well, there are a number of other things that we think we will be doing ultimately with this radar. There will be enough power, for example, that we can see Venus all

the way around in its whole orbit, and so we would look at Venus -- that is, the echo from Venus -- when it is around behind the corona or when it is beginning to be occulted by the sun.

This, I think, will give information on the corona. If we're measuring at the polarization all that time, we will be able to say something about the electron density. We will observe the fading and do the sort of things that they do when the Crab Nebula goes behind the sun, although this time we will be controlling the source.

Another thing that we think we might be able to see -- this is rather more unknown -- are shock waves propagating through the solar system. Some people -- particularly Professor Gold -- thinks that there will be shock waves coming out from the sun, very thin structures, maybe a thousand kilometers thick, or less possibly, and if there is enough of a change in the electron density during the passage of this shock wave we should be able to see it and we should be able to measure the Doppler shift. That is, we should be able to get an echo directly from this front as it comes from the sun, say, toward the Earth; and this is something that we will at least be looking for.

Well, there is, in fact, a whole book -- this is the book -- of these experiments. This is a book that was written a couple of months ago. It is called "Scientific

Experiments for the Arecibo Radio Observatory," and what I have begun to do, almost, is to read some pieces out of this report, and I don't really think there is any point in doing that. Some of you, I think, will already have seen this report, but if anyone is interested in all the little things that we thought we might some day do, he can get a copy of this report.

I think that's enough of this story.

DR. RICHTER: I know many of us don't like to talk about dates when we expect to have results. Some of us are more sensitive about this than others.

Could you hang a couple of tentative dates on these Venus and Mars --

DR. COHEN: No.

DR. RICHTER: -- surface measurements?

DR. COHEN: I attempted to say no immediately and sit right down. Venus is close in the spring of '61, and this machine won't be ready by then, and I guess it's 18 months after that. I think we should have some measurements on the surface properties on Venus, then, at the end of 1962.

DR. RICHTER: Any questions? Discussion?

Next on the agenda for this afternoon are comments from the University of California. I would like to call on Dr. Silver.

DR. SAMUEL SILVER: I want to say that I felt lucky

to be asked to this meeting. I didn't really expect to say anything except to annoy some other speakers, which I have already tried to do this morning, but what I will do, then, given the opportunity, is tell you a little bit about the Space Sciences Laboratory that was set up at the Berkeley Campus.

For the sake of some of my friends here who can think of me only in terms of antennas, I want to assure them that that is one of the things we are not going to do in the Space Sciences Laboratory. It so happens that I didn't start life as an antenna, and I don't intend to end it that way.

What is much more to the point is that this is a laboratory that is inter-departmental or extra-departmental, and was charged to cover all the biological and physical facets in its program; so in the last six months I have been going -- well, I shouldn't say made to do it -- I have enjoyed it very much -- but running around in big circles and little circles learning about viruses and bacteria and physiology, big dogs, little dogs, things of that kind, in addition to trying to find out how one gets money to do some of these things. I am not sure which is the most difficult problem, really.

Well, to come down to being serious about the program itself, as we visualize the laboratory it will

include the work on the Berkeley Campus in the departments of physics, astronomy, chemistry, various parts of engineering that are doing basic research related to the space field, and this would involve some things in electronics, and some things, perhaps, in the fields of air or science.

Work in the biological sciences will involve bacteriology, genetics, physiology, some anatomical matters, things of that sort, related to living in a space environment.

I should clarify very quickly the position of the Radio Astronomy Laboratory of the Berkeley Campus, about which some of you know. It is a separate laboratory under the administration of Professor Weaver; however, we have worked closely together -- he and I -- at the inception of that laboratory, and it is our plan that we will have a number of collaborative programs between the Space Sciences Lab. and the Radio Astronomy Lab. For example, some of the things that we have been talking about is a possibility of tying in with the Cornell project at Puerto Rico, and this is probably news to Marshall Cohen, and perhaps it is even to Bill Gordon -- I forgot if I mentioned it to him -- and with the people at NRL. Now, this one I do know. I think I did speak with Yaplee about this. In doing some studies of scattering from the very edges of the Earth's atmosphere and from interplanetary space, and perhaps beyond. It's purely a question of how much power they have. Weaver

is absolutely opposed to putting a radar system on the California site since he went to so much effort to find himself a relatively noiseless position.

As far as the moon is concerned, the projects in the making are mostly in other aspects than the automagnetic ones. I might mention there is a nuclear analysis project which is being formulated by the people at Livermore in conjunction with some people at STL which will come under the jurisdiction of the Space Sciences Laboratory, but that's way off the subject of the present meeting, fortunately, because I couldn't really say very much intelligent about it.

The other thing that I would like to point out in terms of something which should receive attention is that of the propagation characteristics of electromagnetic waves over the surface of the moon. This is something which will arise in the communication problem if we ever put a couple of beings on the moon, since, as far as the surface of the moon is concerned, the general structure, they very quickly will run out of each other's direct line of sight of communications; and so it is important to know something about the propagation characteristics and try to think ahead as to how one could use electromagnetic waves in communicating between different points on the moon.

Some people feel that it is rather hopeless and they suggest that when we use seismic communication, which

I suppose is also a possibility, and I'm hardly in a position to evaluate that.

Coming to the subject of Venus, the interest in Venus is built around a more general program, which is the extraterrestrial life program, in which the main people involved currently are Professors Calvin and Weaver. We are also getting Carl Sagan as another professor for, I believe, two years, and who knows, it may last longer. He is, as you know, very much interested in planetary atmospheres. The whole idea of this extraterrestrial life program is to try to identify the presence of life on a planet by using the whole gamut of spectroscopic information that is available.

Now, this means that one has to look at a system over the entire spectral range from the ultraviolet clear down into the RF. One has to know something about the behavior of biological materials, particularly plant forms, find the interrelationship between absorption spectra and reflection spectra. They do not happen to coincide. One has to know what the relationship is between the absorption and reflection and emission spectra. One, then, has to be able to establish a sort of library of spectra associated with various forms, or at least with various biochemical components of life; and one also, then, has to know very much about the spectral properties of the atmosphere itself;

so that essentially you can overlay the pictures of the planet -- using pictures, of course, in a very loose sense -- pictures of the planet at different wave lengths, and then see what you can interpret through the entire layer of different spectra observations, so I think what I'm going to do in the case of Venus, I think I'm going to send up in a vehicle sensors in the near infrared, the far infrared, sensors in the microwave region.

I don't know just what to say about the optical and the ultraviolet things at this particular point, but those would have to be obtained pretty much in the same series of experiments if one were to make any deductions out of a composite study of this type.

Of course one of the most critical problems in relationship to this is the chemical composition of the atmosphere itself. If we don't know what's up there it's going to be pretty hard to interpret any experiments, whether they are radar experiments or infrared experiments, passive radiometer experiments, and so on.

I think it is important to plan on radiometers which will measure spectral lines of what may most likely constitute the atmosphere of Venus.

An interesting thing came up in the discussion in our own all-university Space Science Committee about ozone on Venus, and this arose out of a presentation I was

giving of some work that my group had been doing on the ozone in the Earth's atmosphere and measuring absorption in the atmosphere on one of the characteristic lines of ozone. Now, Professor Urey said that he felt that there was, or there is, ozone in the atmosphere of Venus and that it would be well for me to give some thought to doing a radiometric study of the atmosphere of Venus.

Now, in order to do that there are one or two things to do. One is to get up high enough in our own atmosphere so that you are well beyond the major region of absorption and the reradiation from the ozone in our own atmosphere. The other one is to plan a radiometer to go into a space probe which will go around Venus and make the observations.

Now, the nature of the experiments, of course, will depend largely on whether this is to be a single flight around the planet or whether the vehicle will go into orbit. As I listened to some of the things this morning, I think about our own interest and the kind of experiments we would plan. I would say that most of them would be ruled out in the case of a single flight around the planet. At the time of observation it would be too small, it seems to me, to deduce very much in the way of measuring a spectral line via radiometer technique, but of course in a subsequent flight, when one can depend upon the vehicle being in an orbit around

the planet, then I think one can go into a number of very sophisticated experiments, all of which, I think, can be packaged nicely into a vehicle, and some of the things which I just mentioned can very well be done.

I think that's about it.

DR. RICHTER: Are there any questions?

The next comments on the program are from Harvard, and I would like to call on Dr. Lilley.

DR. A. E. LILLEY: I believe we have wandered over a variety of topics, and I think I can do best, possibly, by stressing the case on behalf of the people who are interested in passive experiments.

I think probably it is a 3-to-1 ratio so far of people who are interested in radar over those people who are interested possibly in doing passive experiments.

I think the major point -- I do have one or two things specific I would like to say about the possibility of doing space probe work within the ground rules that were laid down this morning, but I think it is fair to say that -- as you heard when John Bolton spoke just a little while ago -- that every time, without fail, a new technique has been introduced in radio astronomy from the passive point of view rather startling results have come about, and I would like to speculate just a bit about some of the possible experiments that one might do, and some were just mentioned

by Professor Silver.

The transmission of the Earth's atmosphere, as radio astronomers know it, starts in the millimeter range and runs out to where the F-2 layer cross over into the decameter range, and one centimeter is in here, and then there are various which close off propagation due to O_2 and H_2O .

Now, in general there are programs sponsored by NASA and other agencies which are already at work looking in both these regions with the advent of space vehicles, satellites and probes, and with balloons and again later with satellites. Both of these regions of the electromagnetic spectrum which are now forbidden to the study of radio astronomers will open up.

Now, there is another area between here, one centimeter, and out to the decameter range which ground-based telescopes from a passive point of view, such as the two 90-foot dishes are working in, and this region is pretty well covered, and I think one can say that there is a justification for doing experiments in this region only when the signal that is to be investigated in the vicinity of the planet is so low that you are relying on $1/R^2$ factor to get in close enough to elicit the radiation.

Now, the kinds of things that one looks for in this range are the spectrum of the background radiation,

the low frequency outburst from Jupiter. There are at least, I believe now, three separate components from Jupiter. You have heard of one which was independent of wave lengths. It was a low frequency storm burst, and there was undoubtedly a purely minimum emission in the centimeter wave length range.

Now, there is a group already, I believe, under the direction of Doctors Coates and Roman in NASA which is looking into the kinds of experiments one wants to perform in this range and in this range and some of the work that is directly applicable, I am certain, to space probes that go in the vicinity of -- in the immediate vicinity of the planets, and while, from the radar point of view, these misses of thirty or forty thousand kilometers may sound a bit large, from a purely patent point of view or from the radio astronomer's point of view, coming that close to the planet means that the planet attains an angle in the sky of something like 20 or 30 or 40 degrees, and this is fairly sizeable.

Now, to indicate and to simply underwrite how unusual things occur every time one introduces a new technique -- I won't touch on Jupiter, which I had planned to, because Bolton has covered that very well. I would like to come back and say something about Venus and some recent results on Venus that have come to our attention at Harvard and relate that to what one might do in a space probe.

The spectrum of Venus runs from infrared into the radio range and has a peculiar variation of lightness temperature as a function of wave length. In the infrared the temperatures are the order of 300 degrees and out at a three and ten centimeters the equivalent black body brightness, if you use the Rayleigh beam's approximation, of course, what the temperature would have to be, if it were black body, to produce the flux that you observed. The three and ten centimeter point fly out here at an intensity corresponding to a black body of the order of 600 degrees of Kelvin; whereas in the infrared range they are down around 300.

There is one earlier reported 8 millimeter observation near 8 millimeters which was obtained by averaging a large number of curves, and there is some question of whether or not this measurement, which is in the intermediate range, is right; so there is a need for getting these measurements across here; but more recently B. B. Victovich of the Lebedev Institute was here as a guest of the National Academy, and he reported a measurement with the new 22-meter diameter radio telescope at the Oskio Station, which is about a hundred kilometers south of Moscow, and observations made at 8 millimeters. Now, the paper has not appeared in print as yet, but it is very interesting and indicative, I think, of what new measurements mean to you if you were to set out to do an experiment in a space probe.

What you will do two years from now you would probably have to decide today, although the intermediate results from ground-based observations might change the picture before you have time to do it.

He reported the following type of measurements at 8 millimeters. This antenna, being 22 meters in diameter, he said gave him a remarkably good signal-to-noise ratio, and he plotted the position. Let's call this the phase angle between the sun and Venus -- which is swinging past the sun and moving out the other side -- and the Earth. He plotted the brightness temperature, this equivalent black body temperature, that I was just describing -- I will draw this one over here -- as a function of this angle, and what he found was -- although he had no slides with him and merely drew a rough sketch on the board, so I can't put quantitative labels on here for you. What he said was -- at the program which we have at Harvard -- that the equivalent brightness temperature of the planet had a systematic dependence on this phase angle; meaning that after you correct the solid angular size there seemed to be a systematic change of apparent brightness temperature with the phase angle.

Now, the only thing which is changing in his measurements, as the planet moves further and further away from the sun, would be the apparent fraction of the disk, which is illuminated by the sun.

Now, the Soviet interpretation of these measurements is as follows, and I think there are at least two interpretations, so it might be premature to pick this one as the right one; and, in fact, it might, of course, as is always the case, wait for confirmation of these measurements before you pick either alternative; but his interpretation was that these measurements imply that the planet is rotating very slowly; so slowly that the planet, as it rotates out from under the illuminated part, cools off rapidly, and so that there is a true black body temperature difference between the illuminated part of the disk and the dark part, and then one would get, after correcting a systematic brightness temperature change with phase angle.

Of course if you were that close to the planet, under the ground rules we heard this morning, where it is spread out some 20 or 30 degrees, depending on how you passed by, if you had a modest microwave radiometer, it would presumably be quite easy to see that signature if the beam was all right as it passed across the terminator, and the planetary surface being here -- if this is a dark region and this is the illuminated region -- at least one could confirm this type of measurement rather easily in the immediate vicinity of the planet, and I think possibly with crystal video radiometers which can use a transistorized

audio amplifier and the crystal technique that the British used earlier, and it is now being used at ARGMA, one might do this kind of measurement with a system that is reasonably simple.

Now, I mentioned at least possibly one other interpretation. You see the Soviet view here assumes already that the radiation at 8 millimeters is coming from the surface. Now, it's possible that this is a purely atmospheric phenomenon and that some emission mechanism which depends on the illuminated and unilluminated side is responsible for the difference in brightness, and if that is the case one then really needs to go back to this type of plot and know in advance whether or not the atmosphere is truly opaque through this transition region between the infrared and the centimeter decimeter, excuse me, in the centimeter wave length range here.

So it's probably desirable, when one is planning these pay loads which include radar activities, which have been discussed in these wave length intervals, they should be, I think, considered in coordination with some simple passive experiments which might be done at the same time.

Now, I conclude by saying that both the pay loads here and the ones here probably will have been flown a number of times, both in probes and in satellites, prior to the schedules that Dr. Pickering put on the board this

morning, and so that the question of reliability probably will have been settled by that time.

Incidentally, I have -- we were a few miles south of here yesterday checking out on a pay load, and I have some radiometers with me that are in this frequency range that are scheduled for flight, if anyone would care to see them.

DR. RICHTER: Are there any questions?

The next discussion will be from Dr. Straiton of the University of Texas.

DR. A. W. STRAITON: For a number of years at the University of Texas we have been investigating the propagation of millimeter radio waves, and our prime interest is in this millimeter region. We started at 8.6 millimeters and have worked on down to where we have made measurements at 2 millimeters.

In the course of this work it appeared desirable to make the propagation measurements through the entire atmosphere using the sun and moon as a source, so that we set up a radiometer and have used it more at four and three-tenths millimeters than at the other wave lengths.

We have made radiometric measurements of the various materials on the ground, the ground itself, and the reflection from small lakes and sky temperature, and the

like. We have also made observations of the sun and the moon, and measured the apparent temperature of the sun, apparent diameter at 4 millimeters, and the total attenuation through the atmosphere, and the total refraction.

We have recently completed a test in which we were looking for a possible power saturation effect in the absorption spectra by making night and day measurements. We measured the variation of the radiation from the sun and moon, both as a function of angle, in the daytime and at night, to see whether or not there would be any difference in slope indicating a difference in the amount of absorption through the atmosphere. We ended up with as nearly the same slope as we could measure for these two cases.

We have also just completed a radiometric measurement on the sun at two and one-tenth millimeters, and did this with a one-foot antenna, which was a horn with a lens in it. This measurement at four three used klystron local oscillator; measurements at two and one-tenth used a same klystron with the second harmonic for comparison.

We have made measurements in the region between a hundred and a hundred and twenty kilomegacycles of roughly two and a half to three millimeters. This is a particularly interesting region because there are a number of other gases which absorb in this region which are present in the atmosphere in smaller amounts. Carbon monoxide, nitrous

oxide -- I think ozone has a strong line in this region -- and one or two others.

We have a 540-foot spectrometer in which we measure these gases in essentially a pure state to determine some of the line width characteristics in this region.

This was the only area in all of our propagation measurements in which we were able to detect any absorption in the atmosphere other than that due to the oxygen and water vapor, but in that region there seemed to be some which was -- could not be accounted for by oxygen or water vapor.

We are in the process this summer of making a trip to the West Texas McDonald Observatory, not to use the observatory but to get an area of where the water vapor will be substantially reduced and the elevation somewhat increased in order to repeat our measurements at 2 millimeters on the sun, and also to make measurements at the range two and a half to three.

We have recently tried some radiometric measurements on some of the radio stars at 4 millimeters in order to see if we could find them or establish a level which they did not exceed. This is a rather critical region for measuring these because it is approximately -- it may be a minimum if they have distinctive thermal characteristics.

We were able to establish a level, but we were

not able to detect any of the sources at which we looked. We took a quick look at Jupiter at 4 millimeters recently and superimposed a number of stands, and weren't able to see it, but from the other data and our known sensitivity we didn't feel that we had very much of a chance to do so.

We think we may have a better chance to see Venus when it comes around next year, so that we hope in connection with the moon work to continue the passive observations with perhaps a larger antenna, which may give us the opportunity of making a crude map of the moon temperatures at some millimeter spectrum.

We are now doing a small job for NASA on evaluating the possibility of putting the millimeter gear in a space vehicle. We have just started this, and it is quite obvious until better generators become available that this is not going to be practical in the immediate future, but it may within a few years.

In addition to our millimeter work, we had one small diversion into the moon radar work last summer in a program conducted with the Royal Radar Establishment in England. We measured the signal which they put on the moon at 10 centimeters in Austin. We were operating on marginal level of signal-to-noise, but there were several things came out of it. The variations which we saw were not correlated with the variations that they saw at R.R.E. That is, for

the peak appearances of the pulses this was very short pulse, and -- but for peak pulses there seemed to be no correspondence between what we saw and what they saw at R.R.E.

In addition, the first pulse returned from the moon was not the strongest one. This built up over an interval of about 50 microseconds, with the peak occurring consistently at about 50 microseconds. Although it was pretty close to being our limit of measurements, it was a period rather clear to us, and we believe that it is true, that the signals that we received were definitely lower than the ones that they received at R.R.E. by a matter of about 3 db. This work was done with a 28-foot antenna which was put together rather hurriedly. Since then the antenna has deteriorated considerably, so that we are going to have to re-work it if and when we do any additional work in this area.

However, we feel that perhaps our capabilities are greater in working in the millimeter field, and we believe that we can make the best contribution by putting our future effort in working in the millimeter wave length region and working at the shorter wave lengths.

We feel sure that next year we will make measurements of the propagation characteristics of very close to 1 millimeter, as well as continuing the programs at the longer wave length, and we hope, if we are able to acquire a somewhat larger antenna -- we have now a 5-foot

searchlight that we have instrumented from millimeter work -- gives us two-tenths of a degree beam at 4 millimeters -- if we are able to acquire somewhat larger antenna we believe that we would be able to add materially to the scope of our work.

Thank you very much.

DR. RICHTER: Are there any questions or comments at this time?

MR. MARNER: (Collins) What was the frequency of the radar work?

DR. STRAITON: The frequency of the radar work was very nearly 3,000 megacycles.

A VOICE: Pulse length?

DR. STRAITON: Pulse length was 2 microseconds, I believe. 2 or 3.

R. N. BRACEWELL: (Stanford) Could Professor Straiton tell us, or somebody else, what is the largest millimeter wave antenna in the country at the present time?

DR. STRAITON: The largest one that I know of is probably the one that the Naval Research Lab has, 10-foot in diameter, and I believe Sam Silver has one like it. I don't know of any others that are claimed to be good to this wave length. I heard, in an indirect route, that the Russians had made some measurements on antenna in the neighborhood of 72 feet at 8 millimeters, but I just heard

this and I didn't hear any details of the report.

A VOICE: They've even made them at 4 from the same antenna.

A VOICE: This seems to me to be a most appropriate tool for planetary studies, and it is alarming to think that there isn't anything planned for anything bigger than 10 feet, and the Russians have got one 72 feet -- 22 meters, whatever that is in feet.

DR. STRAITON: We heartily agree with you and we have been crying on everybody's shoulder for about two years to try to get us one that was considerably larger, but this is quite a problem in building one that was larger. This one that we got, which was one of these plastic ones made in Wright Field -- it was made with the hope of it being good to millimeter wave lengths, and I believe Stanford is in the process of getting one of these now, and I hope that their's is better than ours; but this has a solid copper sheet on it, and when exposed to the Texas sun it's got a little bit warm, and there was no place for it to go but to wrinkle; and in addition they sprayed it with a plastic material without being careful that the plastic glare was uniform, so that they had -- even if the copper had stayed in place, I don't believe it would have been good to millimeter wave length because of the variations in the dielectric coating.

DR. RICHTER: The revised agenda carries a tentative listing of an ARGMA discussion. As it turns out they were not able to give that today; however, we do have representatives from the Army Map Service here today, and as you heard earlier they are doing some interesting things in the way of lunar mapping studies, and the like, and I would like to give them an opportunity to state their interest along with the rest, and I will call on Colonel Haseman.

COLONEL HASEMAN: With no intent to refute Dr. Richter, it is Haseman, and I am from the office of Chief of Engineers. I am in charge of intelligence and mapping, and I feel a certain amount of curiosity as to why I am standing up here in front of you people, because I am very frank to admit about 75 percent of what's been said here today has gone over my head about 75,000 feet.

I do think, though, that we have certain fields of interest which will be interesting to you, and we have certain viewpoints which may be interesting to you, and I preface my following remarks first by another admission.

As of the present time there is no stated military requirement for lunar mapping, lunar analysis, lunar construction, or anything of a military nature related to

Venus, and I say that there is no such requirement stated as of now, but I will not prognosticate the future.

It seems to me that the work that most of you gentlemen are doing, and some of the work that we are doing, is intended for one of two purposes. Fundamental to it, of course, is the acquisition of scientific knowledge of the origin of the Earth, the origin of life, and the universe, and a vast number of other things that we don't fully understand or don't understand at all. This is scientific knowledge that is fundamental to moving ahead. It must be acquired.

But a second facet of it also is that the studies and research that is going on in your many different organizations is also focused on the arrival of man at a given place in space, and I don't think there is any question in any of our minds but what probably the next place we will arrive in space, transcending the distances involved, will be the moon. After that there will be other planetary bodies, but certainly there are those two fundamental and interrelated concepts that underlie the work that is being done and being discussed here.

Fundamental of this concept is the fact that no scientific or military exploration of any piece of terrain can well be undertaken without maps, and that, gentlemen, is where the Corps of Engineers arouses its interest and its

capabilities.

As many of you know, the Corps of Engineers, is the agent for the Department of Defense for preparation of topographic maps for military purposes for all three services, and where any one of the three services go we feel a basic responsibility to map.

I think it is also evident from many of the discussions this morning and this afternoon that the field of the electronic sensors that can be employed from an Earth base, if any, are capable of yielding data on even our own close satellite, the moon, which is capable of being exploited for making accurate maps. At least the present state of the art does not so permit. The only thing that the present state of the art does permit is to take existing Earth-based photography and exploit it for a very small scale, and that's precisely what we are now doing in the Corps of Engineers.

We have under way a 1-to-5 million scale topographic map of the moon. A 1-to-5 million scale is mighty tiny. It comes out on a single map sheet about this big, square (indicating), but it's the best we can do with what we have at the moment.

Concurrent with that mapping operation in exploiting the very fine geological talent in the U.S. Geological Survey they are making for us a geological

terrain analysis of the moon and a scale of roughly 1-to-3 million. That latter terrain analysis is beyond the compilation state now. It is ready to go into final drafting and reproduction. We are hoping to have it off the press before mid-July, but it is highly doubtful that we are going to make it. It would be awfully fine material to have at a certain number of conferences that are coming up overseas during the course of the summer.

Why the lunar mapping? I think I have mentioned -- I think you will agree that it is going to be needed as and when lunar probes, whether they are manned or unmanned, strike out for the moon. It would certainly be extremely unfortunate if one of these very sophisticated and very expensive lunar probes disappeared into a crevice merely because we had not adequately pointed up these major terrain problems before the thing hits there.

By the same token, where do you land on the moon and what is the composition of the surface, and many of you have spoken on many techniques for trying to achieve this.

We are working on that both through the U.S. Geological Survey, and as the representative of Autometric will mention in a few moments they are working on it with Dr. Siegel from the University of Michigan, trying to find out what's the moon surface made of; what's the grain size; what's the stability of the surface; what's the packing factor;

do you go down to 500 feet of dust, as some astronomers fear; or do you have a tightly packed firm surface in places? These are fundamentally questions to an unmanned or manned probe that reaches the surface of the moon.

Now, in an extension of what we are doing at the moment, we have proposed a plan in four steps which would lead up to the point of placing a probe towards the moon. We are in Phase 1. It is a very small scale and wholly inadequate terrain map, and our terrain analysis. We think that the next phase should include very high-altitude photography, perhaps very high-altitude radar scan; possibly television scan -- the hundred thousand-foot variety -- giving far better resolution if it's photographic camera work were larger scale maps. Beyond that we should start design of the actual pay loads that will be placed in orbit around the moon, and finally those pay loads should be system-engineered, designed, constructed, and then someone uses them.

Mr. Ashenbreuner of Autometric Corporation is here. They are under contract to the Corps of Engineers for certain work that relates to this, and I think he has certain details that you will find interesting also, and if I may I'd like to have him take a couple minutes.

MR. B. C. ASHENBREUNER: Thank you. Let me say a few words about our organization, Autometric Corporation.

We are a research and development company, and one of our main interests and experiences has been with the development of new high-speed mapping and surveying methods and systems applicable to those places and platforms where conventional approaches can not be used, for many reasons. Obviously the moon falls into this category as one of the most challenging problems that we have tackled so far.

Under the current study which we are performing for the Army Map Service, and in which the University of Michigan Radiation Laboratories participate, our organization is concerned mainly with three basic problems. The first one is that of selenodesy or geodesy on the moon, if you like. The second basic problem is the mapping and surveying; and the third one is the map usage.

Now, very briefly, the problem under selenodesy is that that the conventional methods used for geodesy on Earth and the parameters obtained for geodesy on Earth can not be obtained on the moon, and therefore we have to develop systems, selenodetic systems, which will work with those parameters that we are likely to get. If we have a moon-orbiting satellite which is in a nearly circular polar orbit and will stay up there for at least one lunar day at -- shall we say an altitude somewhere between 50 miles on up, preferably around 50 to 100 miles -- as far as the surveying and mapping is concerned, the principal problem there is

that you must have a mapping system -- whether this is radar profiling, a line scan, or a standard photographic television system -- which would preserve geometry, and which would preserve geometry in such a form that you can't transmit it and restore it again.

As far as the map usage is concerned, the mapping of the moon obviously is not fulfilling its purpose if the output can not be used for future lunar missions or circumlunar missions, and therefore aside from a standard type map, which will probably look very nice on an office wall, we have to develop some mapping systems which can be used by either manned or unmanned expeditions. For example, you would like to come up with a system which has a stored terrain model, three-dimensional digital terrain model possibly, and which senses a profile as it goes around the moon and automatically compares -- matches these profiles electronically and defines its position by this method.

Now, the objectives of the map which we are investigating under this feasibility study is -- the first thing, a three-dimensional survey of the entire moon, front and back, and accuracy of plus or minus 300 feet, and this is one thing you can not do if you strictly use data derived from Earth-based experiments, because all the distances which you can use for scale factors right now are known with a fairly poor accuracy, even though the precision of both

radar and optical distance measurements of the Earth-moon distance is one mile. There is a discrepancy between the radar measurements and the optical measurements of roughly six miles.

Now, the second objective which is tied in with a survey, of course, is the surface interrogation by electromagnetic means. This is being investigated by the University of Michigan, and obviously you have to tie this information in with surveying information so that you know where the places where you can get certain amount of it.

Then the third objective is to make a map from this data, how to make the map, what type of map, surface property map, terrain map, and then a topographic map, and then the end product, of course, should be used for the scientific selection of landing sites.

This is all I have to say about our efforts.

Is there anything you want to add?

DR. RICHTER: Any questions?

MR. PAUL E. GREEN, JR.: I'd like to ask Mr.

Haseman a question having to do with statistical -- over-all statistical properties of the moon surface, rather than detailed map.

The radar results seem to indicate, due to a sharp peak in the beginning of the return followed by a slow fall, that there is a tendency for the lunar surface to have a

large fraction of it tilted fairly not much at all from the local horizontal. This is a statistical sort of a thing you get by averaging the electromagnetic energy from a large number of places.

What I'd like to know is, has anybody ever taken all of this optical data and gone over it and taken such an average curve showing what the probability of encountering a slope of a different amount is as averaged over the entire lunar surface? It might be very interesting to see whether this comes out to be exactly the same thing as you get at an optical -- at radio wave length.

COLONEL HASEMAN: To the best of my knowledge no such statistical analysis has been done. The stereoscopic effect that we get in the photography that we are using is due primarily to the libration effect. We can't get long enough base line to take simultaneous photographs for a stereoscopic effect that is worth a hoot, and even with what we have we don't have enough stereoscopic effect to have any confidence in contouring.

We had hoped that we might, when we first started, that we might be able to get something on the order of 500-foot contours. We have given this up but we are going to have to go to form lines.

MR. GREEN: Don't you do this by shadows; get the slopes by the shadows?

COLONEL HASEMAN: You can get elevations by shadow, but when you come to these broad apparently relatively flat areas you can't get any differentiation in elevation across those areas. You could know that at this edge of the area you have got a wall, and by shadows you can find rather accurately the height of that wall, but you don't know the height of the base of that wall relative to the base of some other crater over here, perhaps a hundred miles away.

From Dr. Siegel's work it seems quite apparent that there are many areas on the moon that are reasonably level, reasonably smooth, and well placed adjacent to areas that appear to be of immense scientific interest, but to try to get a contouring effect from what we have, it just can't be done as yet.

The answer really there is no substitute for putting a man on the moon.

One other item I might mention also in connection with this, as a further indication of our intense interest in this whole program, is the Chief of Engineers is at the moment issuing an order setting up a geodesy of its Intelligence and Mapping Research and Development Agency which will take over the R and D effort now at the Army Map Service, the R and D effort now at E.R.D.L., and combine the two scientific talents that are available, the scientific

sources that are available, to focus more closely on this type of problem. There may be a solution to it from ground base photography, but we haven't found it yet.

MR. RICHTER: Any other questions?

A VOICE: I didn't quite get that figure. You said you hoped originally that you might be able to estimate it at 500 feet?

COLONEL HASEMAN: We hoped we might put in a contour interval of 500 feet. (Inaudible.) We thought we could be accurate to 500-foot gaps. We can't do it, not with what we have now. Not with the photography we have.

A VOICE: What do you think you can do?

COLONEL HASEMAN: We could probably do one to a thousand, but this is such a gross interval that it really is almost meaningless. We can get a better indication of changes by using form lines and by taking shadows, and just ignoring the difference in elevation of these broad flat areas. (Inaudible.) It will give a better indication of the service.

DR. RICHTER: We have another representative from the University of Michigan. Dr. Barrett.

DR. A. H. BARRETT: We heard this morning of some of the radar experiments under way at the University of Michigan. I would like to talk a little bit about our radio astronomy program at Michigan, and in particular on some work

that has occupied my time and attention with regard to the interpretation of the radio astronomy measurements on Venus.

To begin with, perhaps many of you know we have an 85-foot antenna at the University of Michigan, and in fact the one which you have here -- the first one that was built here was built with the specifications as written by the University of Michigan, with the possible exception of your high-speed drive motors.

This antenna is instrumented at the present time at 8,000 megacycles with a wide band receiver 1,000 megacycles

band. In addition we have a measure at 8,700 megacycles installed on the antenna, and we have an every day dickey-type radiometer at 16,000 megacycles, and perhaps here I should say that in response to the questions of how large a millimeter antenna do we have in this country, why, this is working at 18 millimeters. Maybe that's pushing it.

There are five or six of the sources that we have indeed detected at 16,000 megacycles. In addition, we can detect Venus. We have detected Venus at this frequency. We hope to detect the planet Jupiter, though we haven't as yet succeeded in doing this. Our beam width is about six minutes of arc at 8,000 megacycles. Hopefully, it will be in the neighborhood of three at 16,000 megacycles, but this has not been entirely checked yet. This is a bit hard to check because the sources themselves are of this size with

the exception of Venus; at least the sources that we were able to detect, and the response that we get from Venus is not all you might like; especially when it comes to determining the beam widths.

Let me now turn to the results which have been obtained from Venus. You heard them discussed a short while ago with regard to the variation in temperature with wave length. It is common knowledge that temperatures at 3 centimeters and 9 centimeters turn out to be approximately 600 degrees, 580, somewhere in there. The temperature in the infrared and in the visible determined from the vibration rotation bands of CO_2 are slightly under 300 degrees. Why this discrepancy? There has been one measurement reported at 8.6 millimeters at the Naval Research Lab where they obtain a temperature substantially in between the two of 410 degrees.

So, this situation is that at 3 and 10 one has 580 degrees. At eight-tenths of a centimeter one has 410 degrees, and in the 8 to 14 micron band -- and I don't know, somewhere around 7,000 angstroms CO_2 , one ends up with temperatures of this order, 285 degrees.

This measurement -- and I put the uncertainty on here for it is indeed quite large -- but this attracted my attention, and I asked the question is it not possible now to take these measurements and hope for measurements in the

future and find something out about the atmosphere of Venus from these measurements, and in particular underneath the cloud cover, for it is well known that astronomers, optical astronomers, are compelled to interpret their results in terms of temperatures at or above the clouds, for they are not able to see below the clouds.

One needs a model atmosphere for this sort of thing, and here one has to go on what is currently available. There seems to be no doubt that the main constituent of the atmosphere of Venus is CO_2 , carbon dioxide. That is at least above the clouds. There isn't anybody who will argue with that. I have assumed that that is indeed the main constituent of the atmosphere underneath the clouds as well.

In fact, when I originally started these calculations, I took this to be the CO_2 , and this much nitrogen in the atmosphere, and the balance here was water. Now, this is a lot of nitrogen, and it turns out the calculations do not depend very much on the amount of nitrogen as long as you do not exceed these figures.

If you went here to 90 antenna, say, you would not alter things very much at all. I have assumed that this is indeed the temperature of the surface of Venus, 580 degrees. I make no attempt to explain why the temperature is that high. There have been theories on this, especially those of Sagan, I believe it is. With this assumption and with this as a

model atmosphere, and dividing the atmosphere up into two parts -- an isothermal part above the cloud layer at this temperature, and an adiabatic part below the cloud layer with this value for the surface temperature -- one then can come up with a reasonable distribution of temperature and pressure versus height, and if this is T over the surface temperature one gets a plot of this order where this would be one (inaudible) 580 degrees down in here, and we hit 285 degrees and we level off. This is not at all unlike -- this is H . This is height. This is not at all different from what we have on the Earth. Then one can calculate the absorption of microwaves with this atmosphere and with this variation of temperature, and you also get here the variation in the pressure. You've got to match gradients and all sorts of things here at the boundary level. This is height of the clouds, incidentally, where we have the transition from adiabatic to isothermal equilibrium, which is 33 kilometers on this model, and not at all an unusual unexpected value.

One can calculate this opacity, and the major uncertainty here is the value of the pressure at the surface of Venus. This you carry along throughout all the calculations and then you plot the radio frequency spectra you would expect to be emitted versus frequency, and you do this for a range of surface pressures, and one finds that in

order to match up the radio data one needs a value between 10 and 30 times what we have here on the Earth for the pressure of the surface of Venus.

Now, this is largely based on this measurement which is a -- has associated with it a large uncertainty. One of the reasons I attempted this work, the reason is a bit wasted on an audience of this nature because it was to stir up interest in millimeter wave measurements, which is hardly necessary here.

These are high. These are higher than you might expect on the basis of the abundance above the clouds of CO_2 . I buy factors on the order of 3, but this is what the radio measurements indicate at the present time. I will be particularly interested to know what the Russian measurement is; in other words, just exactly what is right here or right here; some sort of a scale on this diagram, but we just don't have it at the present time. I would expect it will be in this range maybe 500 degrees, but I think the drop off at 8 millimeters is significant.

Turning now, for a moment, to some experiments one might do in the millimeter range, or at the low centimeter bands from the satellite, there has been water vapor detected, and on the frequency spectra one obtains from Venus -- and I ought to remark that the amount that they have detected above the clouds is not anywhere near as

high as the values I have used in my calculations here. It's really rather small. Incidentally, this is for zero percent. H_2O . And you would need an appreciable amount in order to alter this much, and there just isn't hardly anybody thinks there is that much. In fact the value Strong gets is ten to the minus three percent, something on that order, so it is real small; but on the radio frequency specter -- if this would be Lambert here and this is the temperature that you would detect with a radio telescope -- without the water you come along like this. With the water you have a -- have the resonance just -- this will level off here -- 285. These are the water resonances. One is about one centimeter or so, and one at 1.6, I think it is, millimeters, and you heard from Professor Straiton here that there are a number of other molecules, and indeed there are. There is NO_2 , N_2O , CO , O_2 , ozone. All of these have known measured resonances in the ranges between -- oh, say, one centimeter on down to one millimeter, and I think eventually there will be experiments done from probes to detect these lines.

Initially one would be very happy with just a measurement at the smaller wave lengths, something under a centimeter. In fact, here one might have a measurement at one centimeter and have a frequency doubler in this system so they could get two measurements, and even if you don't

have absolute values if you get the relative values in this range it would be extremely useful.

Thank you.

DR. RICHTER: Are there any questions?

PROFESSOR MARSHALL COHEN: (Cornell) You have essentially a two-component atmosphere you are working with there. (Inaudible.) In the Earth's atmosphere the temperature goes up and down and up and down again. (Inaudible.)

DR. BARRETT: No. In the lower regions, however, it does. There is a drop off as you go up in altitude and eventually you hit an area that is reasonably isothermal. Then above that it starts these odd things, but by this time you are well above the majority of the gas, and the other variations then would not -- would have a small effect on a model of this sort.

PROFESSOR COHEN: Venus is a lot closer to the sun. You might expect Venus atmosphere to be more wildly fluctuated than the Earth's because the heat is applied, is more, and so on.

DR. BARRETT: The recent occupation measurements indicate that it is reasonably isothermal above the clouds. This is the Harvard work that has recently been published.

J. V. EVANS: Couldn't you (inaudible) the question on the grounds that we see (inaudible) type of 8 millimeters for the Earth in the region of the different temperature

that the meteors (inaudible). Why not the same on Venus? Why don't we have clouds, high altitude clouds, there which the principal clouds one sees, in most of the atmosphere below, so that you just don't see (inaudible).

DR. BARRETT: The majority of the gases, indeed, underneath the clouds. The pressure at the cloud layer is three-hundredths of the value at the surface, so that actually what you do above here has very small effect other than the temperature here, and as you raise this up and down you will move the value here that you level off at the real small wave lengths, the order of a millimeter, or even less.

D. E. JONES: (JPL) Dr. Gibson has indicated that the uncertainty on this 8 millimeter measurement now is about a factor of two less.

DR. BARRETT: I hadn't heard that. Does he still buy this value?

D. E. JONES: Yes.

DR. BARRETT: Oh. I am very pleased to hear that, but you think this is now about 80?

D. E. JONES: Yes. He said about 20 percent.

DR. BARRETT: Well, that's nice. At least I am glad it is narrowed down. We need more measurements here.

A VOICE: What would your basis for the selection of 33 kilometers as the depth of the adiabatic portion of your model?

DR. BARRETT: Yes. With the model atmosphere that I had -- and I see I rubbed it off -- you have CO₂ and nitrogen, mainly. If this area here is to be adiabatic region then the gradient of the temperature here is a function will be fixed by the gases and in fact the ratio of specific heats of the gases. Now, this is a function of the temperature for CO₂ and at 580 degrees you would work out that the gradient of the temperature is 10 degrees K per kilometer. At the top of the clouds, bottom of the clouds, at the cloud level, at this value of the temperature it will be 8 degrees. So in the model that I used I just used 9. Average value. Used 9 degrees. And then with that slope when you hit this temperature that just is the height of the cloud level.

A VOICE: Right. These hinge on your basic measurement of 285 or assumption of 285?

DR. BARRETT: Yes, sir, entirely. Entirely. And above this here we have a scale height of the pressure of 6.8 kilometers, I think was the number, which is in excellent agreement with the occultation data above the clouds.

A VOICE: Doesn't the composition of the clouds influence the atmosphere, and if so what composition would you assume?

DR. BARRETT: I don't think it would particularly alter the composition. It may alter the microwave effects,

properties, and I have not allowed this in any way, as I don't really know how you do it. You could assume a certain size of -- I don't know, ice crystal. Well, you have got to assume what are the clouds, and if you want them ice crystals then you have to pick a size.

P. E. GREEN: (Lincoln Lab.) Green from Lincoln Lab. Did you say something about the expected electron density?

DR. BARRETT: No. Not very much. I haven't gotten around to working on that yet. I would not end up on this model. I don't think I would end up with the high values that Professor Siegel needs, but maybe he can comment on that question.

PROFESSOR K. M. SIEGEL: I needed a pressure of 40 atmospheres, if I remember correctly, at the surface.

DR. BARRETT: And then you had rather high values, then, of the density, electron density. Well, we are talking about the same range, and I don't maintain any accuracy for this. It is all based on the model here, which is in reasonable agreement with what has been observed except for the abundance of the gas above the clouds, and here I am disagreed by a factor of 3, but there are no estimates, that I know of, have the pressure at the surface of the clouds.

DR. SAMUEL SILVER: I think the electron density will depend strongly upon the magnetic field situation around

the planet. If the particle radiation can come in, even in the low energy portion of the particle spectrum, then you will get a lot of ionization of all the atmospheric components, and this, in turn, will determine what the effect of ionsphere would be, and I think here again is an area where some earth laboratory measurements are required on attachment combination dates for these gases in the energy range that one would be dealing with in the particle compartment.

PROFESSOR JOHN G. BOLTON: I might say at this point we have attempted to take (inaudible) from Venus and unfortunately, of course, the planet is a long way away, and we have an upper limit of about 1500 degrees, which means that there is virtually no radiation (inaudible).

DR. BARRETT: Is there hope that you can eventually cut this down?

PROFESSOR BOLTON: Yes. By April next year, and the position of the planets makes a lot of difference. We have also attempted measurements on Saturn and Mars and Mercury, and on Saturn we can say that any radiation on the scale of Jupiter has got to be down by a factor of 4. This is again an upper limit.

One of the difficulties with Saturn is that it just moves in and out of the galaxy amongst a lot of other strong sources. Jupiter, of course, we have got to wait almost five years before we can do a north-south measurement

(inaudible) because it is so wasteful looking so far to the south.

DR. RICHTER: We could not break a precedent by having a meeting of JPL without having a JPL presentation. We have asked Bob Stevens of our telecommunications division to tell us a little bit about Goldstone, and let me ask Walt Brown, if we can have some suggestion of what the Goldstone trips will be for those who are interested.

[Discussion off the record.]

MR. ROBERT STEVENS: Forthwith I have some slides here which, to the nine of you who are going tomorrow, will give you an idea of what you are going to see, and where you are going to go.

This slide shows the location. Here is JPL, and there is the Goldstone Test Station. It is out in the Mojave Desert. It is about a hundred miles airline distance from Los Angeles.

Next slide, please?

This is -- we have been asked why we selected this site, and we have been accused of choosing it as acclamating the ground for picture men to inhabit the moon, but actually it was selected because it provided a fairly radio-noise-free environment.

Well, this is Goldstone Lake. It is a large dry lake bed. Here is the site of -- the receiving site.

It is a little shaped area about two miles in diameter. Approximately seven miles away we have recently installed a transmitting antenna which is nestled down behind the hill here. One of the objectives is to provide as much terrain shielding as possible between the receiver and transmitter. Camp Erwin -- this is a military -- it is a training base that I think its largest claim to fame was turning patents, Africa (inaudible) in this area, and it is ideally, -- it is an ideal area for this sort of thing.

Next slide, please?

This is the receiving antenna, and as was mentioned this is similar in almost all details to the Michigan antenna, and it is probably one of the reasons that the existence of the Michigan antenna at the time we set out to start -- that is the desire of the antenna -- for the University of Michigan territory -- one of the reasons why we selected it. It is a polar mount machine 85 feet in diameter. There are a couple differences in the surface, is a measure surface.

Next slide, please?

This is a picture of the operating console at the site, and the operator is sitting in front of the servo control panel, and it is a drive system that is a high-pressure hydraulic system, and can be operated in closed booth by automatic control. The picture -- this is a closed

circuit television system which has the camera along with the objects (inaudible) and we use it for some of our alignment measurements and mostly use it for acquiring optically physical radio targets.

I meant to mention at the outset this station was built and used for space communication work, and was originally -- first time it was used was in the Pioneer 3 moon probe attempt. I will have to confess that it is a dummy picture on the TV screen.

Next?

This is the transmitting antenna which was recently completed and has all configurations to provide a better coverage without the limitation in the polar mountain (inaudible) special limitation leaves a sort of a high-shaped (inaudible) unavailable to it, to the north and below the polar axis, and this -- one of the purposes for this antenna was for use in the project echo method satellite communication experiments. So we wanted to get a better solid angle coverage than obtainable with the polar mount. These sites are all sort of self-contained; have their own power house. I see they have a microwave relay tower there. That is used for communicating between the two sites. (Inaudible.) One of their uses of that link is to provide real time steering of this antenna from the pointing signals of the receiving antenna. This is sent down in a coded form

from the receiving site and there is a -- a digital system which (inaudible) the commands with the outputs and provide an automatic slave mode of operation for this antenna.

Next?

This is the interior of that control building and the transmitting site. This is the type of television equipment. At present there is a 10 kilowatt, 2400 megacycle transmitter at the site.

Next slide, please?

Well, this is a diagram of part of the present equipment. I was going through some slides here of the various diagrams which has constituted Goldstone, and this is probably the most representative of its present condition, and at the top of the system is shown the receiving antenna. There is a 360 megacycle receiving equipment. There is also receiving equipment at 2390, 960. These receivers in general are narrow band phase lock receivers, and the present -- in the present system this 2390 receiver provides (inaudible) channels for generating single tracking signals. Shown here a lock which indicates we can take teletype input data and -- of predictions of a position, for example, in the echo satellite, and (inaudible) predictions we could take this incoming data on teletype and use it to derive the -- to position the attendant.

Well, let's see, the other item I made brief

mention to, that the digital (inaudible) fit into coordinate converter which corrects (inaudible) so with the difference in the access or generation and generation signals to be sent down to the transmitter site for its steering. The transmitter site is pretty straight forward. As I said, the way it was set up was the project echo where the Bell Telephone Laboratories at Glendale were to radiate to us in a 960 megacycle signal which we listened to and we would radiate to them at 2390 signal.

Now, we didn't use that much on the first echo balloon because it didn't get up. We had quite an experiment (inaudible) back and forth.

O.K. Next slide. Are there any more?

Oh. This is just a presentation of the coverage of the receiving antenna for a thousand-mile altitude satellite. This actually represents the orbit of the echo satellite, and this sort of wavy line is cut off due to the terrain masking. There is an average terrain masking of about 5 degrees. (Inaudible) site is located and it is cut off at the top due to the limitation of the polar mountain (inaudible). This other circle happens to be the coverage from about (inaudible) and the intersecting area that is shaded is the area in which communications could be carried out in that particular experiment.

Any more slides?

The data from the -- the tracking data from the antenna is written of in sensitive real time, consists of -- and it can be put on teletype (inaudible) in a tracking operation. The message would be sent to a computing station to calculate on the measurements. As shown there, see the first column of station number. Data condition tells whether or not to make use of this data. The GMT. The hour angle (inaudible) 358 -- 756 degrees and declination and then give a 5-digit number related to the Doppler velocity.

I think that is all I would like to -- there is one more slide, but I think I will drop that because it just indicates the expansion of the message.

Can't drop it now.

(Inaudible.) it will be arranging -- range measurement capability out there (inaudible) which will probably be done this fall. Wide band. Two-way capability for the range, completely coherent. Initially a coded message about megacycle band width.

That I know runs them out. Thank you very much.

PROFESSOR SIEGEL: I was wondering why you settled on a 50-foot balloon. Can you give me some (inaudible).

MR. STEVENS: Well, I guess it is the state of the art with the guy that makes these balloons, O'Sullivan (inaudible) laboratories. It is interesting that this particular size does provide a reasonable voice capability

to send a good quality voice message back and forth with the (inaudible) had about the same sort of equipment and they had a 60-foot antenna, so it does -- happened to correspond to the capability that can give you a message with a good speech message.

PROFESSOR SIEGEL: Do you feel the reliability would go up if the size of the balloon went down; would the number of balloons inside the original balloon went up in -- in other words, supposing they have (inaudible) original, because I know how, for example, to make reflectors that are spherically (inaudible) which very much large radar cross sections than a perfectly conducting sphere, and I -- so, this is not the best way to get the maximum energy back for a given radius sphere.

MR. STEVENS: Yes.

PROFESSOR SIEGEL: And so I was wondering why the choice?

MR. STEVENS: I think -- well, I can't distinguish from cause and effect in the original choice of the thing. If you measure reliability of getting a small balloon up to a -- if I'm not mistaken they are equal. They are zero. They also had in mind the use of this balloon in measurements of drag, so there were some additional experiments to be made along with this.

A VOICE: Was an optical tracking tied in with that, too?

MR. STEVENS: Well, it is hard for me to distinguish because from frequent -- from the design of the optical tracking came in I believe after it was there, and so it was used both at Goldstone. The idea of acquisition on the first passage was to be optically, and of course --

DR. RICHTER: I think you are right. Drag measurement was the first (inaudible).

MR. STEVENS: I was thinking about another comment that I heard that we were going to see that a (inaudible) all the newspapers because it would be quite a thing to see, would be the first satellite really visible (inaudible) human, that we so launched -- was to have been so launched, that it would be visible all the time. I mean it was in the sunlight all the time, so visible at night. So I don't know whether this is really why they chose an 8-foot or not.

DR. GORDON H. PETTENGILL: Isn't it true that a sphere is the only shape which will give you a constant bearing cross section to a wall of bi-static angles which this geometry can present you with? Isn't that the prime reason they chose it?

MR. STEVENS: That is true, I believe.

PROFESSOR SIEGEL: If I can use the blackboard I will show you something that will always give you -- for the same radius or smaller -- will always give you a larger cross section of bi-static angles as seen from Earth.

I will give you three versions of complexity, and I will give it to you on the order which I and a man by the name of Rayburn, who is President of the Autometrics Corporation, thought of.

First of all, if you were trying to get something that had a maximum cross section you would, of course, try to talk about things like lunar reflectors, so if you made a sphere -- and for the first case if you put a metal coating back there -- and I know initially this is a non-aspect independent, but I will make it aspect independent in a moment -- and you made this a Lunberg reflector and make it a defocus Lunberg reflector, you then would get a cross section which would be much larger. In fact, if you want to talk about back scattering a cross section of this would be 4π square of a Lambert square, which is enormous. Suppose you want to make this aspect independent. What you do is you make this layer not a perfect conductor, but a partially reflecting surface. So let's just do the back scattering case, because it makes the mathematics a little easier. For those who have worked with lenses they can do the defocus case and (inaudible) few more terms.

You spoke about back scattering. The Lunberg lens reflector ordinarily would have given you $4\pi A$ squared of the atmosphere, but as soon as you put a partial reflecting coating on it the back scattering cross section -- we will

say the power reflection coefficient is R so that a ray comes up -- another ray comes back -- so we would expect the cross section to look like R times πA^2 , plus a ray which is going in, so it's got a transmission coefficient into the side and reflection coefficient bounces back here; transmission coefficient then comes out here; so this would be $4 \pi A^2$ over λ^2 .

Now, you prefer to optimize this, so you take the derivative of TRT. Dr. Equal to zero. Solve for R . Well, T , of course, is $1 - R$ in this language. Solving for R you get power reflection coefficient is one-third; transmission coefficient is two-thirds. So that if you take a defocus Lunberg lens you put a partially reflecting coating on it, the wave length of interest -- power reflection coefficient one-third; transmission coefficient two-thirds -- you get this cross section, putting in numbers, this is four-twenty-sevenths, and this, then, would be a third.

Now, for example, to give you a feeling for numbers, if you weren't interested in -- say, in 24-hour satellite. You are interested in the 17 degree case, recite those numbers in the second. This will give you a feeling, if you direct back scattering, if you wanted to use this for back scattering, this thing is at X band 50-foot radius gives you a radar cross section which is ten to the sixth larger than a NASA balloon.

If you want to talk about bi-static angles you find out that the -- you've also got 40 db above the NASA balloon for by-static angles, up to about 8 degrees, and from 8 to 17 degrees the increase over the NASA balloon continues to decrease, the latter 17 degrees, you're only about a factor of about a hundred better than the NASA balloon. So the 24-hour satellite would only see 17 degrees of the Earth, but for that cone you always have this factor which is well above the NASA balloon for the same radius sphere.

There are a couple of questions. I prefer to go through the other two generations first, if I can, because the first argument about this thing is that this is a solid, and how are you going to get such a big solid up there, rather than doing it with a balloon, and the answer to that goes as follows:

You make this in successive layers, and when you make this with successive spherical layers you coat these layers with radioactive paints, which you can have as high as a lifetime as you wish. You can choose half a lifetime of 5,000 years. You can choose surfaces which lunarize at these temperatures so it becomes quite hard. You are not going to worry about holes since you have the source in the form of the paint, and it turns out the ideal index that you would get as you go in from the outside into the center, --

the index reflection that you need goes from one at the outer layer to -- now, I will use a normalized radius which I will call R , which is the center, is equal to zero, and is one on the outside, and this index we actually would like in the case of this gas to go down to about a half or a little less, and it turns out the optimum which was graphically (inaudible) numerically something like this, and now what you essentially have is different kinds of charge particles in the successive things and in about eight or nine different spheres you, in fact, can get a gas-kind of filled thing which could actually be blown up, and isn't as -- doesn't look initially as such a high practical problem; solids of this kind of index, and, in fact, that is a very much larger cross section than the NASA balloon.

Gordon, you had your hand up early, so --

DR. GORDON H. PETTENGILL: I just wondered about very much larger angles since the NASA balloon had to handle a forward situation with these angles was greater than 90 degrees.

PROFESSOR SIEGEL: I was thinking a (inaudible).

DR. PETTENGILL: Say, you have a limited cone of back scatter, (inaudible) necessarily the best choice?

PROFESSOR SIEGEL: Right.

DR. PETTENGILL: But in the echo situation it seems that you (inaudible) collection of energy under the conditions

you are faced with.

PROFESSOR SIEGEL: Well, if the maximum of angle were 90 degrees -- See, let me point out what energy you are working on here. I could give you -- I could work out the answer for 90 degrees and give you that optimum, too. I couldn't do it in two minutes, though. If you take a cross section of a sphere (inaudible) we land is much less than spheres, and it looks like this. Where this is at (inaudible) is equal to π the half beam width is 1.6 over KA , when K is 2π of the wave length and A is the radius. Now, if one is interested in bi-static angles, as in the case just mentioned, π over 2 , then one has the energy to play with. This one can't use, so one has the energy to play with, which is here, and that would be based energy, and one can work out the index in this kind of treatment, such that you can gain this energy and put it back here, so that what you are effectively doing is putting that energy back here, and this kind of thing. In the 24-hour satellite case this was down to only 17 degrees, and of course we are building this thing up to a big enormous factor, and we are saving all that energy. This has nothing to do with drag.

DR. RICHTER: We are about out of time, and we hoped to have some open discussion. I think, since we have had discussion as we have gone along the way, and we do have open discussion scheduled for tomorrow morning, I would like

to call on one more speaker. Walt Brown said he might make some comments about some of the early thinking on a spacecraft carry radar for looking at Venus on this fly-by mission, and just so that people will have this thinking in mind when the working group meets tonight, so those of you that don't come tonight can hear it, Walt thought he would go through it quickly.

We are always running a correlation between the names of the people that wanted to attend the classified, perhaps classified, session tomorrow and available clearances, and we will try and let you know that before you leave.

[Discussion off the record.]

MR. WALTER E. BROWN, JR.: The spacecraft radar for Venus fly-by -- actually, I think, the best thing to do is show the slide first, because this describes it. We will talk about some of it just quickly. The reason for this is tonight we are going to have a radar session and a radiometer session here for those who want to do some work, and maybe we can come up with some experiments that we should fly on spacecraft.

Since the people will be separated and this radar that we are planning contains both radiometer and radar aspects, I thought I might just cover it briefly.

[Trouble with the slide projector.]

I will describe it in words briefly. What we

wanted to do was generate three narrow beams that would look at Venus; put three spots on Venus, one in the center and two out towards the limbs. This antenna -- this pattern would rotate 180 degrees for each sample that was made on Venus, and three samples would be made. In each sample there would be a hundred pulses.

Now, the pulses -- the sort of thing that we had in mind was something running about 50 kilowatts peak and about 12 miliseconds in duration, and the reason for the long pulse was that we wanted the pulses from the limb beams to be returning at the same time the pulse from the central beam was returning so that we could mix the frequencies obtained in these three beams and obtain the Doppler toward the limb.

Now, you have components that are contributed, of course, by your rate of change in velocity -- range velocity, as well as rotational components, and if the calculations we made were based on the idea that the rotation of Venus was somewhere between one day and ten days, and for these estimates we felt that the Doppler component caused by Venus, the maximum component, would be about ten percent to one percent of the total Doppler component due to the rotation that you get in flying by, and actually, since the slide projector isn't working, I will try and sketch what the slide looks like.

Now, in flying by this, the center of the distribution at which we are aiming at is ten to the fourth kilometers, and then this has some distribution which is not symmetrical about this. There would be three samples as we go by, so -- and one here.

Now, it turns out that if we use this 50 kilowatts -- 50 kilowatts is the total power -- and three beams means there is less -- there is about -- as I recall the numbers were 42DBW for the central beam and 39DBW for the limb beams.

The signal-to-noise ratio in the central beam for the case where you are at the 45-degree angle for the central beam turns out to be in the neighborhood of 26 db.

The outer beams, of course, we have a problem on because we don't know how fast it is going to fall off. My feeling is that the Venus surface is going to have echo characteristics very much similar to the lunar echo.

Now, as I said, the postulation as to what the signal-to-noise ratio will be out in the outer limbs is based upon the assumption that the behavior of the echo from

Venus is going to be very similar to the behavior of the echo from the moon. It disagrees somewhat with what Professor Siegel might think, but my feeling stems from a different -- a little different approach than Professor Siegel took to arrive at the characteristics of the echo from the moon, and there is actually something equivalent to a modulation loss involved here.

You know that I think -- it was Trexler who brought up the idea of modulation loss in an article some time ago -- actually when you limit the area that you look at you have the same sort of an affair taking place as you do when you limit the pulse. You're only looking at a certain area of the sphere at a given instance of time, and this leads to a smaller -- in a sense a smaller power return than if you looked at the whole sphere.

You would also have this loss if you looked at a narrower band in frequency rather than the whole spectrum that comes back from this sort of thing, so I prefer to call it a loss and aperture loss because it may be an aperture loss or time or space of frequency.

So, the aperture loss that you get for a beam this size is about the same that you get for a 10 microsecond pulse, in the neighborhood of 17 db, and as you go out on the limbs now you have to compute how fast things are falling off, and we don't know whether our assumptions are correct.

Actually, if you look at the Venusian you might postulate that there is some form of precipitation going on with (inaudible) this would be and it probably wouldn't reach the surface of Venus, but there might be large areas of precipitation that would also affect this type of measurement.

Now, we were thinking of doing this at around 10 kmc, and as these beams rotate, this one -- they rotate 180 degrees; then when they get over here they will rotate back 180 degrees; and then over here back 180 degrees this way. On the way out from the Earth we hoped that (inaudible) rate and ^{spin} stem axis of the Earth was just to find out how things are working. And it is a case where, as Professor Reintjes had mentioned before, that we should (inaudible) in tune and ready to go. Of course, by using this technique of the center beam to mix the frequency with, to mix the frequencies from the outer beams with, this helps us quite a bit in being able to have a wide I.F. band, so to speak, and therefore increase the probability that this might succeed. Yet it is a rather cumbersome system at the present. It weighs -- the modulator weighs the most. Probably weighs -- the modulator itself probably weighs (inaudible) 60 to 100 pounds the way we are thinking of it at the moment.

Now, Bendix York made a quick-look hardware feasibility study on this to see what type of antennas we

have. In fact this slide is out of their report, and we will discuss this, perhaps, more tonight. It is one of the things I think we should try and find out, whether it is a worthwhile idea. The point is if it is and we want to do it we have got to start advanced development in another month or so, and that is why it is important to make some sort of a decision pretty soon just how we might go about doing this. The other things that would be measured -- of course we might measure the trailing edge of the pulse (inaudible) off, and to see if there were any humps in it that indicate reflections from two different layers. This sort of thing would also be included.

What the range would be. We could measure range, but not knowing the velocity of propagation in the area of Venus, especially down here, I am not sure that it would tell us too much. It might.

Are there any questions about this? I have just tried to cover briefly, and this is about all the radiometer people will see. Oh, I can show another picture for the radiometer people. (Inaudible.)

The way this thing will be left in stand-by -- the radiometers, of course, will be on during all the time this isn't on, and this is only on for a two-minute interval here, two-minute here, and two-minute here. So it is on for -- it's not too many seconds. A hundred pulses. So the

radiometer will be on, but the wavy beams are left -- the position they are left in as this thing goes by, is something like this. There is a disk of Venus, and the three beams will be left like this, the relative velocity factor being this way. In other words, the beams will be at -- when you are put further away the beam will probably be nearer the pole and it will be bigger on the surface. I don't know if -- actually you will be looking at these three areas as we go by. This will be bigger. This will be bigger. I think that is the sort of thing that happens. So as you go by Venus, assuming Venus isn't rotating at all, you will sort of look at some bands that maybe look something like this. You will also, on this spacecraft, expect to measure the magnetic field in conjunction with this.

A VOICE: I would like to know how much weight this 50 kilowatts transmits.

MR. BROWN: Well, it depends on the pulse width essentially. If you want to take a narrow pulse width you can get down to 20 pounds or so, but with a pulse width we are talking about the weight is in the order of a hundred pounds; but it is a big item and it is going to have a one-meter dish, perhaps, associated with it, and unless we can use the communication dish, but there are problems there

because the communication dish has to be pointing back at the Earth while the solar panels are pointing at the sun, and this dish should be pointing to Venus, and the question of moving things around and using -- using the same dish here with three feeds on it, and so it is rather difficult in this situation. We chose the Doppler, by the way, versus the pulse shape because we felt that it would be less marginal. If we are only looking for something that is ten to one percent of a total effect try and extract this out of a pulse shape. We feel it would be rather difficult because of the statistics.

A VOICE: What was the frequency?

MR. BROWN: The frequency is in the 10 kmc region.